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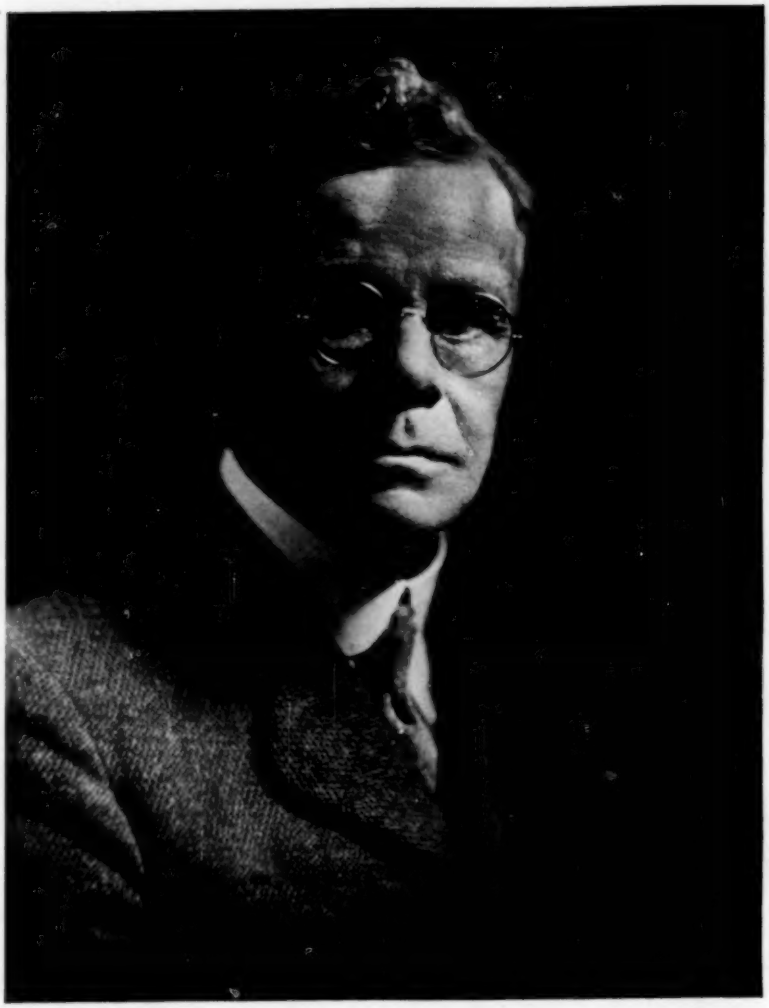

Edmund B. Delabarre

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THE PSYCHOLOGICAL REVIEW

EDMUND BURKE DELABARRE

1863–1945

In the death of Professor Delabarre, American Psychology has lost another of its 'Founders.' His professional life reached back to 1892 when he was called to Brown University, and promptly established the twelfth psychological laboratory in America. That was the year in which the American Psychological Association was founded, and he was one of the original twenty-six members. In 1903 he was high on Cattell's original list of Fifty American Psychologists. After four decades of instruction and research at Brown he became Professor Emeritus, but continued to come to the laboratory daily, rain or shine, for another decade. Two years ago he developed a serious heart condition, and never really regained his health.

Edmund Burke Delabarre was born in Dover, Maine, in 1863. He attended preparatory school in Providence, and entered Brown in 1882. The following year he transferred to Amherst, where he obtained his bachelor's degree in 1886. After a year at Berlin he returned to take his M.A. in philosophy at Harvard. James aroused his interest in psychology, and advised him to study under Münsterberg. He went back to Germany, and was awarded the Ph.D. from Freiburg in 1891. He accepted an appointment at Brown that year, but received a leave of absence for another year of study, this time under Binet. Hence he actually came to Brown in 1892. Münsterberg came to America the same year, at James' invitation, and built up the laboratory at Harvard. In the year 1896–1897 Delabarre served as Director of the Harvard Laboratory during Münsterberg's temporary return to Germany.

Professor Delabarre undoubtedly started his life-long interest in the kinaesthetic sense while a student under Münsterberg. His doctoral dissertation was entitled, 'Über Bewegungsempfindungen.' Soon after he came to America he became quite interested in automatic movements and related phenomena of hypnosis, and wrote a couple of papers on the subject. He devised a suspended planchette for the recording of these movements. One of the 'Studies from the Harvard Psychological Laboratory' which he communicated to this journal in 1898 contained a report by Gertrude Stein on results obtained with this planchette. His interest in movement appeared in another one of the Harvard studies, in which he investigated the force and rapidity of manual reaction. He continued the study of reaction time for many years, and in 1900 developed a Synchronous Motor Chronograph. This instrument consisted of a calibrated smoked drum, driven by a synchronous motor. Appropriate markers recorded 20 or 30 reactions on one drum. Provision was made so that the instrument could be 'cocked,' and would then deliver an

auditory stimulus after a variable foreperiod. I used this instrument in research as recently as 1930. Compared to a modern chronoscope it was only moderately inconvenient, but highly dependable.

The first objective records of eye movements were reported by Professor Delabarre in 1898. He used a thin plaster of Paris cup which clung to the eye like a modern contact lens. A thread attached to the cup made exceedingly delicate tracings on smoked paper. He never reported the extensive experiments he performed with this instrument, but they convinced him that tendencies toward movement were at least as important as were actual movements in our spatial perceptions. He was also very much interested in the contributions made to consciousness by circulatory responses, tremor, and breathing, and constructed most ingenious apparatus for the measurement of these responses. For example, he had a kymograph which fed paper off a roll, smoked it, and passed it under the writing levers at a constant speed. The recording levers gave no radial distortion, and were equipped with constant pressure tips. This instrument could be operated while he read for hours in his study, continuously recording breathing movements picked up through threads taped to his chest and abdomen. It must be remembered that Professor Delabarre was not interested in the movements *per se,* but only as their return kinaesthetic impulses contributed a background or framework for consciousness. Hence the introspective protocols which accompanied these records were full, and he constantly tried to improve them. His studies of *Cannibis indica* and of mescal were prompted by their reputed effect on consciousness, and he likened them to a 'Mental Microscope' which strengthened and accelerated introspective powers.

Aside from occasional notes, very little of his research was ever published. He was not willing to communicate his results until he thoroughly understood their implications. Thus one experiment led to another. The results contributed to his own systematic thinking, but he was never able to integrate all of the facts into a whole that satisfied him. In 1928 he had already completed a book-length manuscript, but he continued to work over it. In another decade he had material for a two-volume work which he had tentatively entitled, 'The Kinaesthetic Framework of Consciousness.' But he continued to revise chapter after chapter, until illness stopped his work. I have never examined the manuscript as a whole, but I have read typical chapters, and have a general idea of his systematic views. He believed that simple sensations are rarely, if ever, directly present to consciousness. A quotation from his early report, 'Influence of Surrounding Objects on the Apparent Direction of a Line,'[1] might almost be taken as a (too) simple summary of his views.

"The seeing of things is not an innate faculty, nor do the eyes serve in any real sense as the 'windows of the soul.' We have to learn to see. The eyes themselves, through their retinal activities, give us only sensations of color. These, alone by themselves, would always remain a meaningless chaos of vague feeling, unapplied to the objects about us. It is through the *movements* of the eyes that the colors are distinguished, that the chaos is separated into parts, and the parts given a definite and orderly arrangement; and the sensations arising from the eye movements and eye adjustments come to stand for exact dimensions and forms of objects about us only through the fact of their close association with the more stable and unambiguous

[1] In *Studies in Philosophy and Psychology; Charles Edward Garman Commemorative Volume,* Boston: Houghton Mifflin, 1906, pp. 239–295. The quotation is from pages 241–243.

movements of hand and of locomotion which we make in handling the objects looked at.

"The object as we see it is a mental fact, not an external fact. It is a visual perception-of-something. What the object as an external fact is like we cannot know directly, but guess at more or less successfully in scientific and philosophical speculation. For practical purposes we do not need to know or care. It is sufficient for us that we treat the perception as if it were itself the external object, and that this procedure serves as a reliable basis for our actions.

"Each such perception is an enormously complex structure, a mass of closely interwoven present and past sensations. Into its composition enter the actually present sensations of color and of eye movements and adjustment, 'apperceived' or interpreted and given body and meaning by the re-arousal of numberless previous sensations of color and of movement which the rich variety of our past experience has definitely associated with each possible present combination of them. Any object, if it is to be clearly seen, necessitates an eye adjustment of a definite nature. The sensations derived from these motor activities recall and imply other definite motor experiences of eye, of hand, and of body. Each such motor complex of present and revived sensations furnishes the spatial feel, into which the colors are filled; and thus the thing as seen constructs itself for consciousness.

"It follows that every detail of our perception of the spatial characteristics of objects must be dependent on some particular form of movement adjustment. We distinguish differences in length, size, shape, distance, direction, position, just in so far as we can distinguish the particular movements and the adjustments adapted to them. This we can assert with much confidence. For though there are those who believe that these things are to be explained by some inborn capacity of the mind, or that they reside implicitly in the color sensations themselves, yet the evidence for this, the empirical and genetic view, is constantly becoming clearer, and is probably accepted as convincing by most observers. But the growth of each one of these details of space perception is intricate and difficult to trace fully, so that each furnishes a problem whose solution can be found only by long continued and varied experimental research."

He worked out many of these details with considerable ingenuity. The modern objective psychologist would probably be impatient with much of his treatment, especially because it is directed chiefly toward an explication of consciousness. But the pendulum may swing back, and it would be unfortunate if his labors were lost to some future workers of kindred interests. I will make an effort to collect a complete manuscript, and file it in the Brown University Library.

In 1907 Professor Delabarre married Dorothea Cotton, of Providence. They had one son and three daughters. Before his marriage he had always shown an active interest in natural history. At the turn of the century he had participated in the Harvard-Brown Expedition to Labrador, serving as its botanist, cartographer, and historian. He had climbed mountains and explored woods, always with his eyes open to the things around him. After his marriage he shared these interests with his family. They spent their summers on a farm on Assonet Neck, in nearby Massachusetts. Here they swam, built trails in the woods, and dug Indian relics. Even after his children had homes of their own they returned to the farm for reunions. This outdoor life may have been responsible for his unusual vigor; at the age of 79 he could be recognized from afar by his brisk stride.

His scientific studies did not stop during these summers on the farm; they were merely redirected to the things around him. He unearthed the remains of an Indian village below tide-level, indicating that the coast was gradually sinking. He accumulated a small mu-

seum collection of carefully catalogued Indian relics. But his most ambitious studies were devoted to Dighton Rock, a small boulder at the edge of a tidewater river. Inscriptions carved on this rock had puzzled students (including Berkeley) for over 200 years. For Professor Delabarre the deciphering of the carvings was a problem in the perception of concealed figures. After a decade of work he decided that the original inscription had been made by a Portuguese explorer, Miguel Cortereal, in 1511. Later carvings, made by Indians, settlers, and even lovers, had complicated the picture, leading to various fantastic explanations. He supported his own interpretation with local Indian legends, known historical facts, and extensive research on earlier studies. In 1928 he published the whole story in a book, *Dighton Rock*. His work attracted considerable attention among historians and archeologists, and won him the title of Officer of the Military Order of San Thiago da Espada, awarded by Portugal.

No account of Professor Delabarre could be complete without a few words about him as a friend and colleague. Even in his later years he remained a clear and vigorous thinker. When we asked it, he gave his help freely, whether the problem was psychological or personal. His wide background and his good judgment made him a valued counselor. But perhaps his outstanding characteristic was complete freedom from pretence of any kind. He felt no need to bolster his ego by petty conceit or false modesty; he had lived a full life, and although he was never satisfied with his own achievements, he was content to let the record stand for others to read for themselves.

HAROLD SCHLOSBERG
Brown University

REPETITION AND LEARNING. II. PERCEPTION

BY THOMAS W. COOK

University of Toronto

In the first paper of this series I made a plea for adherence to the definition of stimuli as energy activating receptors, and attempted to show that repetition, partial identity, and similarity are phases of the same problem, the discovery of significant similarities and differences (2). In this present article I propose to examine those processes most directly dependent upon the stimuli. What are these? One answer is ready to hand though little in fashion, for the structural psychologists made an unusually persistent attempt at discovery and classification. The resulting theoretical structure is familiar: the sensations and their attributes, with association added to account for learning. In this systematic context the relative roles of constancy and change are quite definite. The units (sensations) are provided by local stimulation, and remain constant with constant stimulation or change to new units or combinations of units with change in stimulation. Learning provides connections only. In practice, however, this meant that 'memory' is mixed with perception, i.e., images are called up by association to complete percepts. It is also true that the reduction of perceptual learning to combination of sensations had to be modified, as in John Stuart Mill's mental chemistry and Wundt's creative synthesis. Here the sensations are themselves transformed into something different. But this remained rather an exception to than an example of association, and I cannot escape the conviction that the structuralists kept this latter type of explanation at a minimum.

Now whatever may be said of the merits of sensationism as an explanatory system (and I am far from agreeing with the sweeping condemnation by its opponents), it prepared the way for its downfall as the most fashionable explanation of perceptual change and constancy by a gradual shift of emphasis from sensation to the attributes of quality, intensity, extensity, duration, and clearness, since the latter have much less sales value than the more 'thing-like' sensations. The passing of structuralism, however, was accelerated by a current from another quarter: a logical outcome of functional associationism. It came to be understood that if one assumes principles of association to be indifferent as to content, and to depend merely upon conditions (as frequency, recency, etc.) one may study the problem of learning without asking embarassing questions about the nature and relevance of the content. This trend is prominent in the American functional psychology of learning, and includes many behaviorists and near behaviorists in its tradition.[1] Even Thorndike, whose 'effects' were at first specific processes of satisfaction and dissatisfaction, ended by defining effects so that their only common feature is their temporal position at the *end* of a series, although the two types of effects are still differentiated by persistent vs. frustrated behavior. His continuing interest in content problems is also shown by his late addition of the principles of belonging, availability, and identifiability, but in the main Thorndike re-

[1] The tendency seldom goes so far as to reject distinctions of content altogether. Meaningless and meaningful material, for example, is still distinguished. But actually the distinction is made as little of as possible, and used for convenience in description only.

sembles other functional associationists in putting the emphasis on the connection.

For my part, however, the associationist solution is not acceptable. For one thing, it seems unlikely that the variety of psychological processes should be a matter of indifference in learning. Moreover, the implicit assumption that change is only of function and never of content seems unwarranted.

We turn then to those psychologists who consider classification of content to have some significance in perceptual learning. In this field major contributions have been made by Tolman, Spearman, Lewin, Köhler, and Koffka. In a paper of the present scope I can do no more than recall that Tolman's constants are sign-Gestalts, discriminanda, manipulanda; Spearman's fundaments, relations, and correlates; Lewin's person, environment, topological spaces, vectors, etc. The view sponsored by Koffka, however, has so much in common with my own (because of my debt to him) that its critical exposition is the best starting point for my own contributions. Koffka's theory of perception accepts the best in the Wundt-Titchener tradition, namely, the rigid definition of stimuli in extra-psychological terms. His divergence from the Titchenerian point of view has a threefold basis. (*a*) Discovery of the field effects of stimulation. This meant giving up sensations as psychological elements, since the latter are dependent upon local stimulation. (*b*) Emphasis upon the laws of organization as a second determinant of the form of psychological processes. Koffka thus on the one hand attributes more of perceptual constancy to stimuli and laws of organization, and accordingly less to learning, and on the other hand admits field changes both in things and relations not admissible in sensationism. (*c*) Treatment of the psychical and physiological as a single field, with the physiological aspect the more fundamental, and the psychical the more accessible. Koffka has not followed perceptual classification far, since it is subsidiary to his main problem. But I shall make use of and assume the reader's familiarity with such of his fundamental categories as things-notthings, process-trace, ego-behavioral environment, etc. (7).

I must, however, make a closer examination of certain aspects of Koffka's treatment of similarity, since they are related to my program of attempting to discover the repetitive and changing features of psychological processes.

(1) Koffka tends to use the term 'equality' when he is talking about simultaneous effects, and either similarity or repetition when discussing succession. It is possible, however, that he would agree with my opinion that similarity, identity, and repetition only differ quantitatively.

(2) Perceptual repetition (equality, similarity) he treats in a blanket fashion as a uniting factor, the sign of a tendency toward maximum simplicity.

(3) Wholes may be organized in time as well as space. The similarity between the successive notes of a melody, for example, is one factor in its temporal organization.

Now I believe this treatment of similarity illuminating with respect to both spatial and temporal organization. But on closer inspection some concepts seem to be in need of clarification.

(4) My own view stresses not only the study of similarity per se, but what sort of similarity exerts what species of effect. (*a*) Even in two-dimensional space perception it is far from evident that all similarities have equal aggregating powers. Color, form, shape, and spatial nearness, for example, seem to differ in their effects. (*b*) The relation of similarity to organization is not uni-

dimensional, for organization requires both unity and variety. In simple figures it is apparent that similarity working alone would cause the figure to disappear. Organization sufficient for the existence of a configuration requires at least articulation into figure and ground. And all articulation requires difference (to keep the parts from fusing) and similarity (to keep the parts together). In complex organized wholes it is easy to demonstrate that increase in similarity does not uniformly lead to aggregation. Thus a thousand lines crisscrossed at random make up a badly articulated mass. Increasing one sort of similarity by turning the lines till they are parallel, and arranging these in some regular pattern, makes for a better articulated figure. So in this context increase in similarity in the direction of greater regularity seems to increase articulation and decrease aggregation. (*c*) Proximity is itself a kind of similarity: similarity of position in time and space. Nor indeed are other Gestalt factors such as closure, good shape, and good continuation, distinct from similarity. (*d*) In other than perceptual fields Koffka himself seems to hold a more restricted view. Thus similarity is said to be the major factor in effecting communication between process and trace. And here Koffka emphasizes that it is not similarity of elements, but of pattern (7, p. 463). Moreover, his explanation of the effect of repetition upon the retention of nonsense materials seems to postulate two opposed effects of similarity betweeen materials and methods in successive trials. For repetition is supposed to inhibit memories of individual trials while fixing the pattern (7, p. 545).

(5) It is important to define our universe of discourse. (*a*) In the first paper of this series I excluded stimulus repetition from psychological processes as such, while insisting upon the necessity of exact definition and description of stimulus constancy and change, as a step in the study of constancy and change of psychological processes. (*b*) There still remains an ambiguity, however; the experimenter may mean that the field seems the same or different either to himself or to the subject. Who shall decide (particularly in animal psychology) what the essential similarities are? The physicist has the same problem of sorting out the nonessential from the essential, and I would suggest that if the psychologist is to use the similarities within his own behavioral environment, he can only use them as does the physicist, to formulate hypotheses for research. He must have some other criterion for selection of the *significant* similarities, whether for the purposes of classification or the discovery of laws. On the other hand, I see no reason to follow animal psychology in its attempt to build up a descriptive and explanatory framework on the experimenter's field alone. In fact, I would attribute to this source much of the present blurring of the distinction between geographical and behavioral environment by calling both stimuli. For I would agree with Lashley that "psychological theories based upon the relations of stimulus and response remain sheer nonsense as long as the stimulus is defined as whatever the experimenter puts in front of the animal" (10, 455), if he would alter 'stimulus' to behavioral environment or perceptual field.[2]

[2] The stimulus in the sense of energy-acting-on-the-receptors *is* whatever the experimenter puts before the animal. What Lashley calls the adequate stimulus is defined by the animal's behavior, and since it must thus be inferred in any case, why not re-define it in terms that will most readily fit the data from human perceptual learning? It seems to me that much of the value of Lashley's acute analysis of experimental results is obscured by his use of the same term for two quite dif-

It is of course quite possible that as we go beyond classification and attempt to formulate an interrelated system of 'laws,' our basic categories will have to be restated. Moreover, if the experience of the physical sciences is a guide, these categories will eventually become both more abstract and more functional (in the mathematical sense). But I do not see that this prospect relieves us of the obligation to define the categories with which we are working as carefully as possible. All the more does it demand that we attempt to discover the degree and kind of equality, similarity, or repetition in any given situation.

It is obviously impossible in a discussion of this scope, and for my purposes quite unnecessary, to undertake a comprehensive survey of perceptual likenesses and differences. I shall therefore merely illustrate my viewpoint by examples from three fields: laboratory experiments in sensation and perception as such, perceptual problem solving, and sign-learning. I shall assume the reader's agreement on two points. (*a*) That stimuli, defined as energy activating receptors, are essential factors in both constancy and change of the perceptual field. The emergence of a novel perceptual pattern, the discovery of relations, and the significance of signs, depend upon a favorable combination of old and new in the stimuli. (*b*) In many cases, much is contributed by less immediate factors. For example, a searching or analytic attitude may be a major factor in clearing up a complex field, and many acts of discrimination may be necessary before one fully sees a diagram of the nervous system, or the markings of birds, or hears a symphony. It is amazing, however, how little knowledge we have of the actual course of perceptual change during learning. In fact, without defending some such theory as that of Tolman or Lewin, I can give no generalized account of the role of repetition in perception. In the present context this is not a fatal handicap, since I am concerned only with a point of view and a method of approach. But it does highlight the dependence of the problem of repetition upon the development of a satisfactory generalized terminology for the description of psychological processes.

ferent things. What is needed is not an attempt to re-define the stimulus, but separation of stimulus and behavioral environment.

I. Laboratory Experiments in Sensation and Perception

Much of the experimental data described in texts under the headings 'sensation' or 'perception' has been obtained by observation on or by subjects seated quietly in a laboratory, under the guidance of a contemplative, observing attitude. When it is remembered that these subjects are usually highly intelligent and trained in the use of abstract terms, it will be granted that some caution should be observed in taking the results as indicative of basic features common to all perception. However, if we observe this precaution, the special characteristics of laboratory perception experiments afford an opportunity of studying perception at its best: the calm attitude and high capacity of S makes this a promising point of attack on the problems of perceptual constancy and change.

That the structure of the perceptual field is conditioned by the field effects of patterns of stimuli should have been evident before the advent of Gestalt psychology, but to that school we owe a revolutionary reorientation of viewpoint and a wealth of experimental data. We are, however, still far from a satisfactory knowledge of the influence of experience on these configurations. We may distinguish several aspects of the problem.

(1) Upon what stimulus constancies do constancies in perception depend? With this particular problem great progress has been made. The answer as it has been formulated to date may be found in experimental manuals on sensation and perception, always keeping in mind that these descriptions assume the special sets and attitudes noted.

(2) What kinds of change occur in perception with constant stimulation? This question is difficult of solution, since normally we at least move our eyes or heads, even if we restrain our tendency to examine and manipulate objects. However, we can at least note a tendency (under an approximation to such conditions) for perception to disappear. But to what degree this is due to sense organ adaptation, a special kind of change in attitude, or some other complex of factors within the psycho-physiological field, I shall not attempt to decide.

(3) What changes occur with successive presentations of the same stimulus? The perceptual consequences of such representation of the same stimulus on different occasions (what is usually meant by repetition) are far from simple phenomena, since we must at least take account of an intervening period of no stimulation, and at the worst of a complex of interfering or facilitating events. It cannot be expected, therefore, that we shall be able to arrive at a single comprehensive statement about such repetition. One hoary problem, however, intrudes at once: the problem of meaning. On first thought, one might be inclined to say that meaning is a common sense term with no parallel in modern psychological theory. But on the other hand, we find Boring stating that Gestalt psychology "does not have a theory of perception because it holds that meaning is given immediately in phenomena" (1, p. 19). Boring himself favors Titchener's context theory of meaning, particularly when reinterpreted in behavioristic terms. To my view, however, the problem of meaning has many phases. Here I shall merely try to sort out and put aside those aspects which are unrelated to perceptual repetition, and deal briefly with the remainder.

(*a*) Let me first attempt to formulate some pertinent features of the Titchener and Gestalt views of meaning, since I think that Boring mixes his own post-Titchenerian development with the former and overlooks one essential feature of the latter. Since Titchener taught that meaning may be 'carried by' sensations, images, or physiological traces, the essence of meaning cannot lie in the specific nature of the context. It must therefore lie in the fact of context: that is, in association by temporal contiguity. A sensation 'becomes' a perception by the addition of any other sensations that are occurring at the time. A particular meaning is thus a purely chance affair, but will commonly at first be largely kinaesthetic, since some movement is always taking place. If then Boring is right in saying that Titchener discovered that meaning is a relation, the relation is one of accidental spatial or temporal contiguity. Now, I think that there is some truth in this theory. Provided we assume certain other essentials, contiguity does give rise to at least one kind of meaning, as I shall attempt to defend in more detail in a subsequent discussion of sign-learning. On the other hand, however, the kind of meaning that is logically (though not genetically) the simplest does not concern the juxtaposition of two processes in the behavioral environment. Recognition, for example, may require only a process and a trace. That is to say, it may occur on the representation of an identical stimulus after an interval. But recognition is not a simple function of stimulus repetition.

The stimuli need not be identical; partial identity of the right sort may be more effective than complete identity. Moreover, recognition is a function of the organization in which the object appears. Also, in common with other forms of meaning, it tends to diminish with repetition of identical stimuli. As Titchener pointed out, conscious meaning tends to appear in general during the development of other novel features of the subject's field. Later much of it is carried 'in purely physiological terms' (13).

(*b*) Members of the Gestalt school use the term meaning in another sense, which I have so far avoided, and which is quite foreign to Titchener. The division here comes from a conflict between the presuppositions underlying the two theories. If one makes use of units whose irreducibility lies in their simplicity, as does Titchener, one can only have a context theory of meaning, since meaning always implies at least the duality of sign and significance. But if the irreducibility of a process lies in its uniqueness of organization, as in Gestalt theory, the meaning of a 'part' may be the part-whole relationship. Moreover, as Köhler has pointed out, a *whole* may be meaningful in the sense that it has internal organization. Thus the universe may be meaningful in Köhler's sense, but not in Titchener's (3).

(*c*) The relations between repetition and meaning in this latter Gestalt sense, then, belong to the problem of organization, some aspects of which I shall take up later in reference to Gottschaldt's experiments and perceptual problem solving.

(4) In what ways can stimulation be changed in successive presentations without perceptual change? This is a complex problem with many ramifications in learning experiment and theory. If we consider stimulus shift, we have the concept of substitute stimuli; if stimulus reduction, Hollingworth's redintegration concept; if stimulus removal, the problems of imagery and memory, appearing in a special form in the abnormal field in the wealth of observational material gleaned from the borderline between illusion and hallucination. The majority of cases cited under this head, however, do not actually concern constant perception with changed stimulation, but constancy of some later consequent with change in both stimulus and perception. They therefore belong more properly under sign-learning.

The relation to Gestalt theory of the facts that are basic to Hollingworth's redintegration concept is best exemplified by Gottschaldt's well known experiments. Since so many accounts of these are available I shall give only the briefest of summaries. The experiments consisted in investigation of the effect of repeated presentation of smaller 'a' figures upon recognition of a given 'a' figure when it is later presented as a part of a larger 'b' figure. Gottschaldt found that in most instances without the co-operation of a special searching attitude such repetition had no effect upon recognition (4). The particular phase of the problem in which Gottschaldt was interested is the nature of the perceptual field, and I accept his general conclusions regarding sensory organization, and of the slight effect of any repetition which does not follow the lines of the existing organization. When, however, we ask what is repeated in Gottschaldt's experiments, we find that he has only made a beginning on our problem: what likenesses between successive trials affect what sorts of learning? An insistent difficulty here is to decide in which of our three systems of concepts we are moving at a given time. Certainly there is some stimulus repetition; the stimuli for each

'a' and its corresponding 'b' contain identical features. Duplication of 'a' in 'b' is achieved by measuring lines and angles, so that the 'a' figure can be superimposed upon the corresponding area of the 'b' figure. But there is also duplication of 'a' in 'b' within the experimenter's behavioral field. A great deal of the surprise evoked by Gottschaldt's results comes from the fact that with a suitable attitude E can see the smaller figure in the larger. My program, however, only admits this kind of repetition for the formation of hypotheses. Repetition can only be said to occur in S's field when reported under controlled conditions or deduced from his behavior.

Is, then, the searching attitude the only constant factor intrinsic to the psychological process of discovering 'a' in 'b'? No; there are similarities between 'a' and 'b' that are quite evident to the subject, and some of these similarities are essential to the recognition. Experimental techniques for discovering the role in recognition of similarities and differences between the 'a' figures presented alone, the 'a' parts of the 'b' figures, and the other parts of the 'b' figures, have been developed in Gestalt work on the perception of figures and groups. In fact, though these studies were primarily concerned with cognition (perception), in all probability the similarities and differences found to be crucial for primary organization of the field would be significant also for recognition.

(5) What are the interrelations and interdependencies between changes in stimulus patterns and perceptual field? Genetically this is the primary problem. The child or animal learns by doing, and learning by doing involves dynamic interplay between movement and perception. Strictly speaking, however, both stimuli and field tensions must be considered. Now, the latter can only directly effect changes in sensory quality or spatio-temporal organization within narrow limits. Illusion and hallucination are abnormal phenomena. Ordinarily field forces change perceptual form and quality indirectly by effecting movement, which in turn changes the stimulus situation. However, except for the manner in which field forces effect movements, the correspondence between movements, stimuli, and field constancy or change involves no new problems. But the possibility of internal transformation of the field deserves more discussion, and some of the problems involved will be considered in the next section.

II. Perceptual Problem Solving

Two quite opposed views are current regarding the nature of non-language problem solving by animals and humans. On the one hand we have the trial and error theory, which meets the problem of novelty and repetition by treating the units as constants, and making their order of arrangement, and their selection and elimination, the only novel features. Although this theory has been applied also to thinking, at that level the units are word or sentence meanings, and depend upon language. At the level that I call perceptual the units are usually said to be movements. Köhler and Koffka discuss this theory at length and since I agree with them, I shall not further consider it here (8, 9).

At the opposite pole lies Spearman's theory, which takes as typical the most sophisticated case, where the subject sets himself the task of singling out and responding to a relation as such. Spearman's constants are elementary apprehensions called 'fundaments,' and his novel features the emergence of relations and correlates. Later the educed relations or correlates provide the constants for eduction of higher relations

and correlates. Although Spearman writes as if eduction occurs only as an instantaneous mental act (error is due to intrusion from memory), his conceptions could be fitted into the context of a study of repetitive functions in learning, if he would admit that in many cases fundaments must be repeated before eduction can take place.[3] But to see why Spearman's theory will not provide a general answer to our problem I must bring the term relation into the context of our earlier discussion. Where are the supposed relations, in the stimuli, in E's field, or in S's field? Certainly there are relations in the stimuli. In fact it is usually convenient to formulate energy changes acting on the receptors in relational terms. But these relational categories are external to the subject and do not change with learning. They cannot therefore be the relations which are discovered in perceptual problem-solving.

Again, derivation of relations from what E sees in the situation is also necessary in order to formulate hypotheses for research. We may arrange the external situation so that to us it seems to be graded along what Hilgard and Marquis call a 'logical or conventional dimension of difference' (5, p. 200), and then set up experiments to decide whether S's choices indicate that he can respond to these relations. But even here one must be cautious in drawing inferences regarding the nature of S's field. For example, Köhler's chicks are sometimes said to perceive relations or even to form concepts. But Köhler does not draw such conclusions from his experiments. What Köhler did was to arrange conditions so that absolute and relative choice were equally motivated and equally favored by stimulus conditions, and then note what type of choice the chicks spontaneously made (3). The relations are of course in the stimuli (along with all sorts of other relations), but, more significantly, in E's field. But does not the discovery of relations as such involve an effort of abstraction only possible with the aid of language and special attitudes? As a matter of fact, there is considerable evidence that not only relations but also things are absent from primitive perceptions. Köhler, for example, states that ape language "can only express emotions, never designate or describe objects" (9, p. 305). And when in the light of such evidence we reëxamine our adult behavioral fields under problem-solving stress we may well find that neither things nor relations are as much in evidence as are functional and dynamic properties, stresses, and directions. It is quite in accord with the facts that only goals and obstructions have thing-like character.

What are the determinants of the constant and changing features of these structural processes? It is possible that constancy and change of simple structures such as light-darkness are contingent wholly upon stimulus constancy and change, and that repetition affects chiefly their sign-function, one end of a gradient acquiring a positive and the other a negative sign. But it is also entirely possible that the structural properties of the field as a whole may be affected by experience in such a way that new patterns and new configurations may emerge. But even if we proceed upon Gestalt lines we have only

[3] Actually Spearman is interested in other aspects of the general problem of repetition or similarity. (*a*) His work is based on the assumption that if individuals make the same relative scores on two tests, these tests measure the same ability. (*b*) As regards eduction, type of relation or 'material' is irrelevant. Whether these generalizations are or are not grounded in fact need not concern us here. They are at least plausible and so illustrate the fact that the search for psychological constants may be made at many levels.

the beginnings of a solution of the relation of novelty to repetition. Just what do we mean by a new pattern? Certainly not one entirely dissimilar to the old. The goal may be little changed, the obstacles are still in the field, even if they have somewhat altered their function. What sorts of transformations are possible and favored by what sorts of conditions? The point at issue may be clarified by a statement of its relation to Koffka's distinction between repetition of process and repetition of accomplishment. The latter occurs when the subject re-reaches a (physical) goal, the former when an organized or reorganized pattern recurs. I quite agree with Koffka's statement that repetition of accomplishment does not guarantee repetition of process. But his further statement that "If, in accomplishing X, I have not *performed* A, although I have *accomplished* it, this performance will have no practice effect for the future accomplishment of A" (9, p. 538), overstresses the discontinuity between old and new. For the statement is either a tautology, *i.e.*, a process must occur before it can be repeated; or it means that a process can only be affected by a preceding process in so far as the two are identical, a conclusion not as yet warranted by the facts. Accordingly his formulation does not solve our main problem. It does not tell us what part repetition plays in the emergence of a new process. The most desultory observation should suffice for the not entirely startling conclusion that some degree of constancy in the initial process is necessary, and on the other hand that change in some part aspects of the total pattern should favor reorganization. Thus in the Köhler problem of the box with the open end, some apes who were unable to find a solution at 180° did so at 135° or 90° (9, p. 233) and later transferred the solution to the 180° problem. Similarly in the two stick problems, it might help if one stick moved slightly toward S just as he picked up another (9, p. 174).

III. SIGN-LEARNING

All perceptual organization has a successive as well as a simultaneous aspect. With zero time the organization disappears. But in many organizations one or the other dimension dominates, and we are here concerned with those dimensions in which succession is the important feature, partly because these have more duration and less extension, but also because the forces of organization with which we are concerned are in the time dimension. So a square is organized in space and a melody in time, while a moving picture has both spatial and temporal organization in more equal measure.

Now, the temporal organization of a melody depends to a degree upon internal repetition, that is, upon the regular recurrence of similarities between its successive notes. But there also occur meaningless sequences in the sense that earlier events have no logical or intrinsic relations with later ones. And in these instances, also, granted the necessary dynamic context, the latter part of a process-in-sequence tends to become a consequent of the earlier part.[4] It should be noted that the process as a whole need not be meaningless. A green door may mean the road to food. But the point is that nothing except environmental accident requires the door to be green rather than red. So the green color has no unique significance

[4] The difference between a rhythmic series and a two term sign is not that the first has internal similarity while the latter has none. It is rather that the recurrence of a pattern sets up expectancy of itself, without any earlier experience of the total series. A single term, on the other hand, can only carry a sign function based either on innate meaning or previously experienced consequences.

in this context except that indirectly derived from the fact that it always comes before the event. Much of our sequence learning is of this sort, and in view of the illogical nature of the succession of events as they actually occur, we would be in a sad predicament were this kind of learning not possible. Such sequence learning has had a favored place in association theory, particularly since the invention of the nonsense syllable and the method of paired associates. It seems to me, however, that its true home is in perceptual sign learning, and I would formulate the most general case as follows: Every perceived object or event that has in the past been regularly followed by another object or event tends to acquire a sign function, and on the recurrence of the first part of the process the sign function tends to operate.

The principle involved has much in common with Tolman's expectancy. As I see it, however, there are two main differences. In the first place, since the sign is in the perceptual field and the thing signified is outside the field, I find it necessary to bring in the concept of traces. If we call the first and second phases of the process P1 and P2, and the corresponding traces T1 and T2, then the sign function of P1 (on its recurrence) is due to the organization of T1 and T2. In the second place, I prefer the term sign-function to expectancy, because the latter seems to imply too high a grade of conscious anticipation, and because expectation varies quantitatively both with the rhythmic character of the previous pattern and with the specific nature of the after-effects. A Wagnerian overture may for all listeners raise anticipation to great heights, and yet the emotional experience may be so various as to arouse many degrees of attraction or antipathy in regard to the prospect of a recurrence. That is to say, there are many forms of temporal organization depending upon intrinsic relationships within psychological processes. But by sign function I mean to direct attention to organization dependent to a much greater degree upon sequence alone.

Since I believe that sign-learning presents the most favorable field in which to discover a general effect of repetition independent of content (and has therefore been the bulwark of association theory), I shall attempt to clarify what I mean by sign-function by applying it to a specific body of experimental data, the so-called associative inhibition or negative transfer experiments with paired associates. Here I cannot follow the custom of calling the first term of a sequence S and the second term R, since I believe that greater clarity in thinking is achieved by reserving S and R to signify physical stimuli and muscular movements occurring in the geographical environment, and thus outside the dynamic, organized context of psychological processes (2). Accordingly, in order to bring the facts denoted by the negative transfer paradigm (new R to old S gives negative transfer) within a dynamic psychological context, it must be restated in terms of process and trace. To repeat: the original sequence P1–P2 is experienced in succession in the perceptual field. This sequence forms a trace T1–T2, and on recurrence of P1 the trace is activated and tends to arouse P2. If P3 actually follows P1, interference occurs. It seems to me that this statement has several advantages over the usual S–R formulation.

(1) The negative transfer is more accurately located, not in the stimulus but in the psycho-physiological field.

(2) The locus of interference within the field gains in definiteness. Change in the second item causes interference because P2 and P3 (or T2 and T3) are incompatible.

(3) The process-trace formulation places the negative transfer emphasis where it belongs: in the first transfer trial. If someone puts a stone in my shoe, I get negative transfer from previous experience because when I attempt to put on the shoe I act as if the stone were not there. But in experiments based on S–R terminology the negative transfer occurring on the first transfer trial is either not measured or lumped with the data from subsequent transfer trials. And since the first transfer trial introduces a new factor P3, whose trace T3 tends to cancel the effect of the interference, such a procedure tends to hide the fact that when the first term is in the perceptual field and the second has to be supplied from 'memory,' change in sequence causes interference in direct proportion to the original learning. An extreme example of the opposed working of the two factors may occur under conditions described in (6) below. If I see the wall of a room accurately painted to simulate books and bookshelves, I will attempt to get a book from it, but I may learn enough from that one trial so that I will never repeat my mistake.

(4) Negative transfer occurs not only when P2 is changed, but when P1 is confused with other perceptions. This is a common case: a child responds to the elevator door in a confused fashion because the door looks the same whether the elevator is going down or up. Whereas it is only necessary to single out the red light that means down from the white light that means up, in order to avoid interference. So negative transfer tends in general to diminish with analysis and discrimination. But this holds only for situations in which P1 is followed by P2 and P1a by P3 *as a rule*. If both P1 and P1a are usually followed by P2 no amount of discrimination will help when P3 is substituted for P2.

(5) The variety and kind of organization in which P1 belongs will affect the transfer. Gottschaldt's 'a' and 'b' figures would only interfere with each other under exceptional conditions. If a part appears alone or in a well-organized whole, its recurrence in a different organization may evoke no tendency toward the recurrence of the usual sequent.

(6) If the situation is very simple we may have 100 per cent negative transfer on the first transfer trial, but no overt interference on the second transfer trial. Incidentally, we have here one of the most striking differences between rat and human learning. The simple situations that Hunter's rats took several hundred trials to reverse might well be responded to correctly by humans on the second transfer trial (6).

(7) It should finally be emphasized that a comprehensive study of repetition in sign learning involves the relation of sign and sequent to the whole behavioral field and to the Ego. For example, it concerns the effect of attention and other attitudes upon the learning of meaningless sequences. Of course this is only in accord with present practice. What is not usually understood, however, is that these processes come under the concept of repetition just as definitely as P itself. Any attempt to apply the concept of repetition to the learning situation cannot logically avoid this conclusion. Thus Thorndike's law of effect concerns the repetition of a satisfying or annoying state of affairs, and when he says that 'mere repetition' has only slight effect upon learning, he means repetition of P1–P2. (He calls them S–R.) But if the effect has existence as a psychological process it can be repeated, and it is this repetition plus the repetition of S and R which in Thorndike's view causes the learning (12).

It should perhaps also be noted, although I must defer detailed discussion of the point, that I do not regard the development of sign-function as dependent upon the after-effects of P1–P2. Sign-function is the general effect of temporal sequence as such. The influence of after-effects in the Thorndikian sense, on the other hand, depends upon their properties. So sign-function and effect are not exclusive but complementary principles. In fact, one reason for emphasizing the first transfer trial is that this is the only place where sign-function can be studied uncomplicated by the influence of specific after-effects.

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AN EXAMINATION OF SICKLES' 'PSYCHO-GEOMETRY OF ORDER'

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I. Introduction

The question of what gives rise to form and coherence in the visual field is one that has engaged the attention of many investigators. Of late an attempt has been made to demonstrate the essential electromagnetic character of the visual field by an identification of the properties of the visual field with those of the electromagnetic field. In this endeavor Sickles has been foremost (11, 12, 13). To establish the equivalence of the visual and electromagnetic fields, he has proposed a psycho-geometry of order which he asserts successfully demonstrates the intrinsic identity of the two. It is the aim of this paper to subject this claim to critical analysis in order to discern to what degree this identification of the two fields may be considered as solidly established.

II. The Transfer of Concepts

It should be expected from the very nature of Sickles' proposition that arguments based on physics and mathematics should loom large and that an attempt should be made to bring to bear on the problem all the decisive conclusiveness that we are accustomed to expect from a telling use of physics and mathematics. As has been pointed out on other occasions (4, 5), great care must be exercised in the transfer and application of non-psychological concepts to theoretical psychology, particularly if there exists the need to modify and adapt them to the problem at hand. If the greatest caution is not exercised, such concepts become not only meaningless and misleading to a degree, but, worse, they befog what mistakenly they seem to clarify and thwart or retard thereby the development of rigorous theory.

It is an elementary proposition to assert that, if one is successfully to carry through such an application of non-psychological concepts to psychology, at least this much should be demanded: that the concepts so transferred should everywhere be discussed in those terms and subject to those conditions which originally defined and gave operational meaning to them (3, pp. 96, 515). Otherwise they become convenient and pliable idea-symbols which obligingly perform however we wish them to. The failure to observe the necessary care in the transfer of non-psychological concepts into psychology inevitably results in the introduction of a sequence of non sequiturs which must involve difficult incongruities of idea and expression as well as an undesirable haziness of argument. A survey of Sickles' contributions to the solution of the problem of the visual field tends to suggest a consistent failure to observe the requisite precautions demanded of valid concept transfer. This is strongly indicated by Sickles' usage of the concepts and terminology of physics and mathematics in a manner both foreign and contradictory to common practice there.

For example, a mathematician would not be inclined to say, *"Any given line between any two points, when devoid of all other determining lines or points, is an impossibility, a non-existent thing possessing neither qualitative nor quantitative characteristics.* It can neither be straight nor curved, because it lacks determining coördinates. . . . Any line,

in vacuo, has to be determined by at least three points in order to have any characteristics whatever" (13, p. 409). Do not two points determine a line? Does not projective geometry conceive of a line as a set of points, even if only two in number (15, p. 236), and develop much of its theory without frames of reference (15, p. 174)? Are mathematicians concerned with 'qualitative characteristics' of lines? Are not any three points in general non-collinear and determine a circle sooner than a straight line? And what can a vacuum have to do with the truth of a mathematical proposition?

A mathematician and physicist would be frankly perplexed by the following: "Properties which can be demonstrated for a given figure [in projective geometry] are directly dependent upon the rules under which the figure was produced, *i.e.*, the initial *order* involved. *These principles of order underlying all mathematics are also the basic laws underlying all natural processes.* . . . Why should all the natural sciences display so close a correlation between the structural and the functional? Simply because everything that exists moves, and everything that moves describes a path with respect to a given coördinate system. It is a vector. The shape of this path is determined by the geometrical laws of the inner forces which determine the movement plus the outer conditions under which it takes place. . . . Whenever a thing functions, the forces determining the function will also determine a configuration. This configuration . . . will correspond to the map of any other identical function irrespective of location. . . . *all identical movement and functions, whatever their intrinsic nature, determine identical configurations which are subject to the geometric laws inherent in the existence of the forces involved*" (13, pp. 418–419). The mathematician would be perplexed because he knows that the concept of geometrical order is unnecessary for the development of much of projective geometry (15, pp. 14, 60) and, further, that finite spaces are derivable in which neither order nor continuity exist (15, pp. 94–95, 201–202). The physicist would be perplexed because he is getting to understand more and more, especially since the advent of quantum theory, that it is *he* who imposes mathematical order and law on natural phenomena and not nature; because he no longer regards force as an indispensable concept, explicitly dispensing with it in both relativity and quantum theory; and because in quantum theory he radically modifies the classic role of the coördinate system for the sub-atomic world, questioning its operational meaningfulness there (4; 5, pp. 266–267, 279).

III. The Electromagnetic Field

The 'fuzziness' which inheres in Sickles' use of the language, concepts, and conventions of both physics and mathematics makes it difficult to pin any statement down with definiteness of meaning and renders debatable every step of his argument from the initial assumptions to the culminating conclusion. It will be, however, unnecessary to argue every point in the sequence of Sickles' demonstration. It will suffice to show that the fundamental contentions of his theory are not what he makes them out to be and, hence, are invalid. Essentially, the theory which he propounds may be briefly stated thus: Any visual field possesses order when objects are so arranged in it that an identically structured electromagnetic field would demonstrate a high degree of coherence and unity (12, p. 197), and it is precisely the electromagnetic attraction between parallels which is the mortar holding that visual field together (12, p. 195).

Overlooking the evident semantical impropriety which permits Sickles at all times prior to proof to speak of visual and electromagnetic fields as if they were anything more than linguistically related (11, p. 85; 12, pp. 192–193), what is to be said of this whole proposition? Does this attraction really have in physics the significance which Sickles attaches to it, and then by extension a similar significance in psychology? An examination of the electromagnetic field produced by an electrical current will provide from a consideration of first principles a direct answer in the negative.

When an electrical current flows through a straight wire of infinite length an electromagnetic field is set up which is cylindrically centered around the wire. Any perpendicular cross-section of this cylindrical field will produce a family of concentric circles, each of a constant intensity. The direction of the field is such that, if the current-bearing wire is grasped by the right hand with the thumb pointing in the direction of the current, the fingers will encircle the wire in the direction of the electromagnetic field. This means that the direction of magnetic intensity at any point is always tangential to the circle on whose circumference that point happens to be located (2, p. 306; 1, pp. 139–140) and, hence, by elementary geometry is perpendicular to the diameter of the circle drawn to that point.

Far from suggesting the tri-dimensional Cartesian system of coördinates, X, Y, and Z, which Sickles makes so much of (12, pp. 192–194, 198; 13, pp. 411, 420), the electromagnetic field suggests rather cylindrical coördinates as the most natural. A Cartesian coördinate system determines as fundamental three families of mutually perpendicular plane surfaces, each family being parallel to one of the coördinate planes: XY, XZ, and YZ; whereas a cylindrical coördinate system is fundamentally related to a family of co-axial cylinders, a family of planes intersecting in the common axis of the cylinders, and a family of planes at right angles to the axis of the cylinders (1, pp. 21–22; 6, pp. 16–18; 8, pp. 587, 589). The cylindrical coördinate system is manifestly the only coördinate system which coincides everywhere with the electromagnetic field set up by an electric current flowing through an infinitely long straight wire. From Sickles' standpoint which should be then considered the most natural coördinate system: the Cartesian or the cylindrical? One should think the cylindrical.[1]

However, the whole question as to which coördinate system brings about the most natural or 'perfect directional division of space,' 'both subjective and objective' (12, p. 192) is at once unreal and obsolete, as Euclidean space may be handled through any variety of coördinate systems (14, pp. 50–51). It is in general a physical rather than a mathematical problem to choose the one coördinate system out of the many at one's disposal best suited to facilitate the solution or understanding of a problem under study (14, p. 13).

Great emphasis is placed by Sickles on the fact that two parallel wires carrying an electrical current in the same direction will attract each other, from which fact he argues that parallel lines in a visual field will tend to bring order and coherence to that field because of a similar attraction (12, p. 195). But this is analogically illusory, as it is easy, if one were to follow an identical argument, to show that parallelism can disrupt as well as unify a visual field.

[1] Sickles, however, states, "Every electric current sets up a surrounding electromagnetic field with direction of action identical with those of a perfectly standard Cartesian coordinate system" (13, p. 420).

First, it will be necessary to show that the attraction between parallel wires carrying currents in the same direction is not a fact of primary significance as Sickles seems to feel, but that it is a phenomenon derivable from the character of the *cylindrical* electromagnetic field mentioned previously. A brief discussion will disclose why such an effect must take place. There is no corresponding analogue in the visual field and, hence, this attraction is not transferable to any exposition of properties there.

Let A and B be two parallel wires of infinite length perpendicular to the plane of the page and carrying current away from the reader. The plane of the page will then contain a perpendicular cross-section of two cylindrical fields. It is evident that between the wires the intensity of the combined fields is weakened, as the two fields are there in opposition; while to the left of A and to the right of B, the intensity of the combined fields is strengthened as there the two fields reënforce each other. This results in a field between the two wires which is weaker than that which surrounds them both. As lines of force behave like stretched elastic cords, the result is to force the two wires together (9, p. 572). Experiment shows us, furthermore, that in distorted fields the electric conductors are always urged sidewise toward the weaker field. It is no surprise then that a mutual movement toward each other should take place (2, p. 308).

Now, Sickles' argument depends wholly on the mere fact that an attraction between parallel wires bearing current exists plus a particular definition of parallelism as lines of identical direction which he has formulated (12, p. 194). This definition enables him to set up a verbal equivalence between parallelism in the visual field and parallelism in the electromagnetic field, as the sameness of direction of the current is essential to the binding character of the field. But suppose we were to define parallel lines without reference to an outside reference system as sameness of direction implies. Suppose we were to define parallel lines in such a way that reference is made only to themselves and reference to direction eliminated. Will Sickles' argument in this case stand?

Let us then define two given straight lines in a plane as parallel if it is possible to find a third line such that this third line is perpendicular to the given two.[2] There is in this definition no reference to direction, but it is a very effective one, nevertheless. In discussing the effects of two parallel wires carrying electrical currents, one is then free to consider the cases: one, where the currents are in the same direction, and this we have already done; second, where the currents are oppositely directed. A brief discussion will show that the two wires will repel each other. Analogically, to use Sickles' method, this ought to disrupt the visual field.

Let a wire, A, carry a current away from the reader perpendicularly into the plane of the page, and a parallel wire, B, carry a current in the direction of the reader up out of the page. Again the plane of the page will contain a perpendicular cross-section of the two cylindrical fields. It is seen that the intensity of the resultant field between the two wires is evidently reënforced, while the field to the left of A and to the right of B is weakened. As movement is always from the stronger field into the weaker, the wires are repelled (9, p. 572). Now, why should *this* contradictory case be ignored, merely because Sickles' wording of a definition on

[2] This definition will be recognized as a familiar theorem in Euclid's system of geometry.

parallelism verbally suggests the other case and limits consideration to it alone? The lines are, after all, still parallel whether they are identically or oppositely directed; whether the angles they make with an arbitrary reference line is θ or $180° + \theta$. One must conclude that nothing has been demonstrated by Sickles in this connection.

Similarly, it may be shown that there is no particular significance to be attached to the fact that, because a perpendicular is the shortest distance from a point to a line, the paths of least resistance in a field of equal tension must be perpendicular to the field (13, p. 418; 12, p. 193). This is a non sequitur, as why should it not from primary considerations? Let a wire, carrying a current directed upward from the plane of the page, be placed in a uniform field such as is produced near the center of the space between the poles, N and S, of two wide magnets. Bearing in mind that the magnetic lines of force extend from the N pole to the S pole, it is readily seen that the wire must move across the field at right angles to it as movement is always from the stronger to the weaker part of a field. To the left of the wire the field between the magnets is reënforced, whereas to the right it is weakened; hence, the movement to the right and at right angles (9, p. 571). Whatever connection there may be between this phenomenon in the electromagnetic field and the something which Sickles discerns in the visual field has yet to be demonstrated.

A word or two should be said about the formula adduced by Sickles for computing the magnetic force produced by a straight current at a distance of r units from the wire. The formula may be stated as $H = 2I/r$ where the current, I, is measured in electromagnetic units, and H is the intensity of the field (12, p. 195; 6, pp. 294–295). Sickles everywhere fails to point out that this simple formula, known as Biot and Savart's law, is only true provided the straight wire is long enough to be regarded as of infinite length when compared to the radii of the circular lines of force under consideration. Otherwise, H is given by a more general formula [8] where the field will vary inversely as the square of the distance instead of the first power as it does when the wire is straight and infinitely long (9, pp. 565–566, 568–569). What all this means for the identification of the electromagnetic and visual fields has yet to be indicated by Sickles. It is difficult to anticipate what the implications could be.

IV. SYMMETRY

One is puzzled at the particular significance that Sickles attaches to the straight line and symmetry (12, pp. 190–192). Sickles states, "With respect to the forces which constitute the known world, a straight line may be defined by the following theorem: the path of least resistance taken by any force through any constant medium between any two points, is said to be a straight line. Because space is a constant medium within the limit of human activity, the straight line is also the shortest distance between any two points" (12, pp. 190–191). But neither mathematical nor 'natural' space is to be regarded as a medium and as such constant, unless one wishes to resurrect the æther concept, long discarded, and identify it once more with space. And not straight lines, but geodesics are the natural paths of particles in the universe (5, p. 267; 6, p. 208). Geodesics assume the form of straight lines only at

[8] $dH = (kIdl/r^2) \cdot \sin \alpha$, where dl = distance element along wire; r = distance of dl from unit test pole p; Idl = current element; dH = increment of intensity experienced at pole p; α = angle formed by dl and r; k = constant.

a distance from large masses of 'gravitating' matter.

As for the straight line being "the only division of an area which can result in symmetry" (12, p. 191), many examples which show marked order in the psychological sense may be given where such is not the case. Even the various spirals, while not symmetrical in the strict sense of the word, most certainly feel symmetrical and possess, moreover, a high degree of that something which is called order.[4]

Furthermore, the role assigned by Sickles to symmetry and the correlative straight line, which makes man's entire evolution dependent on such considerations (12, p. 191), is at variance with modern knowledge, since asymmetry appears early as an integral part of vital phenomena. For instance, Oparin states, "Chemical asymmetry is an essential phenomenon in the structure of living organisms as it is a most characteristic property of all living substance" (7, p. 128), and even Pasteur, anticipating in a way the modern biochemists, believed chemical asymmetry to be the only property sharply differentiating between the chemistry of dead and living matter.

V. Experimental Verification

Two actual proofs are cited by Sickles to demonstrate the electromagnetic character of the visual field: a quantitative analysis of the Ponzo illusion (11) and the 'plus-sign phenomenon.' The same reasons which make it difficult to accept Sickles' demonstration of the identity of the electromagnetic and visual fields make it very difficult to discern just what Sickles does prove in the case of the Ponzo illusion,[5] so that this first proof may be, therefore, dismissed as at best inconclusive. Citation of the 'plus-sign phenomenon,' however, where a distant light when viewed through slitted eyelids breaks into a blurred cross of light, allows of no misunderstanding, for it is an assertion concerning a phenomenon which any one can experience immediately for himself. It will be easy to show, accordingly, that Sickles' citation of the 'plus-sign phenomenon' is the result of a complete misapprehension of the nature of the phenomenon involved (12, pp. 194, 198). The 'plus-sign phenomenon' is an instance, not of the 'vertico-horizontal reference system' inhering in an electromagnetic field, but of the diffraction of light.

Light shows wave-like properties so that under proper conditions it is possible to demonstrate the existence of interference phenomena characteristic of such properties; for example, waves of light out of phase will cancel each other out and produce a pattern of alternating light and shadow. This is exactly what happens when light passes into a dark chamber through a narrow vertical slit and falls upon a screen inside. Instead of a replica in light of the slit on the screen, as one would ordinarily expect, there appears a horizontal band of vertical bars of light and shadow in alternation (9, p. 446). This indicates interference phenomena and demonstrates the existence of wave-like properties in light. The phenomenon referred to above is called diffraction.

If a narrow slit is thought of as made up of two edges mainly, a small rectangular aperture, which consists of two vertical and two horizontal edges,

[4] Sickles' feeling that only the straight line can divide an area symmetrically takes account only of axial symmetry, but radial symmetry results in very symmetrical subdivisions of areas also (8, pp. 83–85). Examples of curves capable of symmetrically dividing circular and rectangular areas are $y = \sin x$ and $y = ax^3$.

[5] This illusion refers to 'the phenomenal difference in the apparent sizes of stars seen at the horizon compared with those higher in the sky' (11, p. 85; also 10).

may be thought of as made up of two slits superimposed on each other—the one slit vertical, the other horizontal. We should then expect a horizontal band of diffracted light owing to the two vertical edges and a vertical band owing to the two horizontal edges. A cross of diffracted light should accordingly appear (9, p. 448). When a lamp bulb or any other light source is viewed through nearly closed eyelids, such a diffraction phenomenon is then to be observed owing both to the slit between the eyelids and to the slits between the eyelashes (2, p. 537). It is this cross of diffracted light which Sickles avers demonstrates the 'vertico-horizontal reference system of the electromagnetic field.' However, it is readily seen that Sickles has misconstrued its true nature.

Sickles asserts further that "to prove that this 'plus-sign phenomenon' is not dependent upon the nature of the light or the background, the head can be tipped to either side, and the directions of radiation will also tip" (12, p. 194). This statement on the independence of the phenomenon may be disproved in the following simple fashion. Look at a bright concentrated light source through the mesh of an umbrella or any other texture in which are small and regular openings. A cross of diffracted light will appear even when the eyes are kept wide open. The orientation of the pattern of light emerging will depend upon the angular position of the edges of the tiny rectangles of which the mesh is composed. By rotating the texture through which the light is seen, the cross can be made to rotate in company *independently of any movement of the head* (9, pp. 448–449; 2, p. 537). This simply and effectively disproves the independence of the 'plus-sign phenomenon' in the visual field and underscores further the diffractive origin of the phenomenon.

VI. CONCLUSION

Sickles has failed to accomplish the projected identification of the electromagnetic and visual fields through his psycho-geometry of order. The theoretical presuppositions are debatable and at variance with what is commonly held as true in both physics and mathematics. Parallelism has been shown to be as disruptive an influence as it is unifying in the visual field. Experimental verification is either lacking or unconvincing. In view of the fact that despite the exercise of much effort and ingenuity the proposed identification of the electromagnetic field with the visual field has not taken place, the absence of substantiating theoretical argument and experimental evidence would seem to indicate that attempts at such identification are unlikely to lead to any definite results along the lines pursued by Sickles. The striking coherence and unity of the visual field, it appears, has yet to be explained.

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AN INTERPRETATION OF STUDIES USING EXTRANEOUS STIMULI IN LEARNING

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Various interpretations have been advanced in regard to the rôle of extraneous stimuli in learning situations. Though certain trends have been shown, significant critical ratios have been rare in comparing group differences for human subjects under such stimuli as shock and/or bell on right and/or wrong responses in learning. The inconclusive and even contradictory results in these studies (14, 10, 8), may be traced in part to the fact that the extraneous stimuli are playing more than one rôle, *i.e.*, an extra stimulus may so operate that its total effect is both to increase and to decrease errors. According to the particular combination of stimuli employed and to the place applied (as on right and/or wrong responses or indiscriminately), there may be summating or canceling effects. As Rexford (11) and Tolman, Hall, Bretnall (14) earlier suggested, the effects of these extraneous stimuli are neither single nor equal and not always in the same direction, sometimes increasing, or again decreasing errors. It is the purpose of this paper to illustrate how the different influences of two extraneous stimuli (bell and shock) may be working together, or in opposite directions, in the particular learning situation for human adults called the punch-board maze.[1]

The five possible effects of the particular stimuli that will be considered are not to be taken as the final or complete interpretation of the rôle of these stimuli. Beginning, however, with Tolman, *et al.* (14), the following influences have been referred to by experimenters using these extraneous stimuli: (*a*) Both bell and shock may have a 'marking effect,' *i.e.*, they may *emphasize* a response, whether right or wrong, and accordingly either decrease or increase errors (14, 2). (*b*) They may be followed by avoidance responses, *i.e.*, the subjects may be *motivated* to escape them (14). (*c*) The extraneous stimuli may be *disrupting*, *i.e.*, they may act as emotional stimuli and increase errors (14, 8). (*d*) They may have an *'awakening'* effect, increasing the general alertness of the subject (7). (*e*) Finally they may have an *informational* effect which would decrease errors with subjects capable of receiving previous instructions as to the 'meaning' of the stimuli, *i.e.*, whether the stimuli followed the 'right' or 'wrong' responses as defined by the experimenter (3).

If some method of measuring directly the effect of these extraneous stimuli could be devised, and error scores of individuals on different combinations of shock and bell be intercorrelated with these measured effects, a factor analysis might reveal the weights to be attached to the stimuli as motivators, disruptors, etc., though, of course, such an analysis might not reveal factors identifiable by such names as used above. In lieu of such an exact analysis, Table I has been constructed to show how much weight might be attached to bell or shock according as the extra stimulus was on the right or wrong responses in a punch-board maze.

[1] The punch-board maze was chosen as, beginning with Tolman, *et al.*, twenty groups under various combinations of shock and bell have been put through this particular learning situation.

We have considered a hypothetical situation, *i.e.*, the weight of these stimuli in determining errors when the subjects had no knowledge concerning the meaning of them, but when the experimenter had nevertheless marked responses as errors or successes according to *his* decision as to whether bell or shock responses would be considered correct. However, with human subjects information as to the correct responses is given at the beginning of the learning trials. And thus the motivation is changed. The subjects are then set to eliminate those holes that are incorrect and to repeat those holes that are correct, no matter what extraneous stimuli are present as they make their responses. Emphasis, general alertness, disruption continue to be by-products of these extra stimuli and to affect the repetition of responses, but the information concerning the meaning of the stimuli provides a new motive to repeat only the responses which will end the learning situation. This new motive cancels in part the motive to escape the stimuli; in fact, the influence of the motive to learn even provides the tendency to seek, if *necessary* for learning, the otherwise avoided stimuli. When the subject knows that the shock and/or bell are on the right responses, he also knows that it is to his interest to learn the problem as quickly as possible, for 'the fewer the trials the smaller the total amount of shock' (or bell) that he will have to receive (14, p. 612). Of course when the extra stimuli follow the wrong responses, the subject, even without the information concerning their meaning, has been correctly motivated to escape them. The information adds nothing to his motivation, or at least we have so considered his learning situation.

Another set of guessed weights from those shown in Table I could, of course, have been made,[2] but justification for the particular weights chosen is now given.

[2] The size of the numbers given in Table I is not significant except in relation to each other. Lines *c* and *d* could be reversed and not affect the final weights, but it is the guess of the writer that the disrupting effect of the stimuli is greater than is the awakening effect.

TABLE I*

	Bell		Shock	
	Correct Response	Incorrect Response	Correct Response	Incorrect Response
Without information concerning the meaning of the extra stimuli in learning.				
(*a*) *Emphasis*	−7	+7	−3	+3
(*b*) *Motivation*	+5	−5	+10	−10
(*c*) *Disruption*	+3	+3	+24	+24
(*d*) *Alertness*	−1	−1	−3	−3
With information concerning their meaning in learning.				
(*e*) Change in motivation with *information*	−12	0	−25	0
Final weights†	−12	+4	+3	+14

* A negative sign indicates decreasing errors, a positive sign the reverse.

† An algebraic summation of these scores may not represent the correct method of combination.

(*a*) As emphasizers, bell and shock will bring a repetition of responses with which they are associated. If the experimenter calls the response incorrect, the repetition will be an error; if the response is called right, then the repetition will lead to decreasing errors. Hence the plus and minus signs in Table I on the line marked Emphasis.[3] It is assumed that bell should have more weight than shock as an emphasizer, since the latter might be followed by a diffusion of response rather than a direct marking effect. A greater weight is, therefore, used for the bell than for the shock ($\pm$ 7 contrasted to $\pm$ 3).[4]

(*b*) As motivators we have assumed that both bell and shock have a negative valence and that subjects not possessing any information as to their meaning would attempt to escape them by not repeating the responses with which these stimuli were associated. In the hypothetical situation where the experimenter is calling bell and shock *correct*, though without the subject having this information, the escape from such stimuli would be called errors. When these stimuli were associated with *incorrect* responses, then an avoidance of the repetition of such responses would be called successful. It is believed that shock has a greater negative valence than bell and hence the former is more heavily weighted in Table I ($\pm$ 5 as contrasted to $\pm$ 10).[5]

(*c*) That shock may have a disruptive effect on the repetition of responses has been acknowledged since its early use (6) though the extent to which it is disruptive would vary with the situation (1), the subjects, and particularly the amount and method of administering. On the punch-board maze, with the usual amount administered in the experiments reviewed, our guess is that the shock is rather highly disruptive, preventing the repetition of responses whether called right or wrong. Exactly why shock is disruptive is, of course, uncertain. One factor that seems to be apparent in the disrupting effect of shock on the repetition of responses is the emotional excitement and/or increased tension that follows shock.[6] That a bell may be a distractor,[7] though without the greater disrupting effect of shock, is considered in giving bell the much lower weight of + 3 in contrast to + 24 for the disrupting effect of shock on the repetition of responses.

[3] Bernard and Gilbert (2), and others, consider that the 'marking' effect of stimuli would decrease errors whether given on the right or wrong. Such an interpretation seems to combine both the informational and emphasizing effect of the extraneous stimuli. The significant aspect would not be simply the 'something happening' after the movement, but the movement as 'correct' or 'incorrect' according to previous information as to the meaning of the stimuli. See Courts (4) and Stephens (13) for opposite points of view on the effect of 'something happening' after a response.

[4] It might be argued that more intense stimuli, as shock, would have a greater emphasizing effect; that, therefore, shock as more intense should have a greater weight as an emphasizer. But with shock stimuli, of the intensity used in these experiments, it is the guess of the writer that the more general bodily effect of the shock would prevent a fixing of the cues, spatial, kinesthetic, or whatever is necessary to serve later as cues, for the repetition of previous responses.

[5] It is assumed that bell has a negative valence. This assumption could, of course, be questioned, even as the negative valence of shock is apparently questioned by those subjects that will *report* it as 'pleasant.' Certainly the valence of any stimuli might be modified when the subject associates them with the *correct* responses. See p. 184 of (2) for a discussion of shock and bell as 'punishment' and 'reward.'

[6] Bernard writes that shock in learning of a verbal maze was especially disorganizing because of "fearful expectancy which preceded it and lack of constructive outlet for tension it aroused" (1, p. 75).

[7] It is possible that the disruptive and distractive effect of these stimuli should be treated separately.

(*d*) Shock reinstates the *general* attentive attitude and thus facilitates repetition by providing the effective setting for the operation of fixation. That bell may so act is also possible and we have chosen to weight slightly both bell and shock with − 1 and − 3 respectively, as indicating their influence by 'awakening' the subjects.

(*e*) When information is given concerning the meaning that the stimuli are to have in learning, the subjects who are to repeat responses associated with shock and bell (bell-right and shock-right situations) will now be motivated to seek these stimuli, and will be increasingly motivated to repeat the responses associated with these stimuli in order to escape from the total learning situation. As learning proceeds, shock and bell must appear more frequently; the negative valence which these stimuli would still possess, would now be in opposition to the motivation to finish the learning.

Organisms not responsive to verbal stimuli would not be immediately motivated to decrease their errors in order to escape as quickly as possible from the shock-right situation. They would not know at the beginning of the trials the problem to be learned. Certainly rats during their early learning of a shock-right discrimination problem will escape shock by running into the 'incorrect' alley, *i.e.*, 'incorrect' from the point of view of getting to the food. The greater number of early errors of these animals in the shock-right situation (16), as compared to shock-wrong, suggests that with them motivation as escape from shock is working as is indicated at first in our Table I in the signs given to the weights opposite Motivation. With the dominance, however, of the motive to get food the animals also reverse their responses in the shock-right situation, seeking shock rather than running away from it. With the human, the information given in the directions concerning the meaning of the stimuli in learning provides the necessary learning motive to overcome finally the tendency to avoid the stimuli of negative valence, adding likewise an effect of its own. Hence the informational effect is indicated by using − 12 and − 25 on bell-right and shock-right respectively, making possible the total weights of − 12 (bell-right), + 4 (bell-wrong), + 3 (shock-right), and + 14 (shock-wrong), as well as the scores seen in Table II for various combinations of these stimuli.

Our interpretation of the action of these extraneous stimuli can only be justified by agreement between the resultant scores from these weights and the actual findings in group experiments. Tolman *et al.* (14) first used the punch-board maze under BR, BR–SR, BW, BW–SW, and although no mean errors were significantly different, the order of the groups was as stated above. Muenzinger (10) repeated the experiment of Tolman, and as shown in Table II added certain combinations of shock and bell not used in the California experiment. Though admitting the disruptive effect of shock, he argued against any other influences of these extra stimuli with human adults on the punch-board maze. In so doing he ignored the possibility that the various effects of these extraneous stimuli may be masked. In fact some of Muenzinger's combinations, as bell-right and shock-wrong, particularly illustrate how the effects of these extraneous stimuli may be in opposed directions. Other studies with the punch-board maze are also shown in Table II: Hulin and Katz (8), MacPhee (9)—three patterns used at each learning situation—and Silleck and Lapha (12). The group results, in terms usually of errors per 20 trials, are shown in the fourth column. The trend is from bell-right as superior learning to

bell-wrong and shock-wrong as the least efficient. When the mean errors are correlated with the theoretical score, obtained from the weights in Table I, a coefficient of .62 is obtained. When the means are adjusted for differences in the learning abilities of the college students represented, the trend is more apparent.[8] Using all twenty groups, the correlation between the adjusted means and the theoretical score is .77. When the bell-wrong of the Silleck and Lapha study is omitted, the correlation is .88, based upon 579 subjects.

Though it is apparent that all the factors determining the group results have not been considered, the size of these correlations support the assumptions that have been made in regard to the working of these extraneous stimuli and indicate that our guesses as to the approximate weights that should be assigned to their effects, when used with the punch-board maze, have been in the right direction. The rarity of significant critical ratios for the differences between the means in these studies may be traced in part to the contradictory rôles that these stimuli play.

We have considered these stimuli from two points of view suggested by Dashiell (5): (*a*) their effect as isolated stimuli and (*b*) their effect when the subject accepts them as related to the final goal of learning. It might be argued that the effect of these stimuli cannot be isolated from the motivating directions which set a learning problem for the human subject. Wallech and Henle (15) deny that reward *as such* has an effect of favoring recall and they emphasize the learning motivation, *i.e.*, the meaning of the situation itself which directs behavior in a particular way. Such stimuli as shock and bell, however, might produce more varied reactions than would the stimuli used in their experiment, *i.e.*, the words 'right' and 'wrong' spoken and one-half cent given. The writer has assumed, at least, that the extra stimuli of bell and shock may produce some reactions which are not related to the learning problem. Some of the disruptive reactions in a shock situation indicate that the learning problem is intermittingly shelved. Also as producers of avoidance responses, these stimuli when on the right may lead to repetition of reactions that are directly contrary to learning, unless the learning of the problem set by the experimenter has been completely accepted as the only method of escaping from the stimuli. Likewise we have assumed that the emphasizing effect of these stimuli may be independent of the goal of learning some particular pattern. Moreover, the general alertness set up in the subject by the presence of bell or shock in the situation might provide the effective setting for the repetition of any response, right or wrong, that provided a release for the motive of the moment. However, the information given in the directions to the human subjects, as to whether bell or shock is correct or incorrect, changes the entire situation, not only bringing about a realignment in motivation so that there is a seeking of the hitherto avoided stimuli, but also very probably changing in some manner the effects that we have listed under the headings of emphasizing, disrupting, awakening.

It should also be remembered that differences among individuals would exist in the complex of motives that may be carrying the subjects to com-

[8] Recognizing that there would exist group differences in the learning ability within the different colleges represented, the writer arbitrarily selected the Colorado groups as standard and added to, or subtracted from, the other groups the average difference between them and the appropriate Colorado groups. The mean difference of 16.6 between the California and Muenzinger groups was added to each California group. 7.6 was subtracted from MacPhee's study done at Carolina, etc.

TABLE II

POSSIBLE VARYING EFFECTS OF EXTRA STIMULI ON PUNCH-BOARD MAZE LEARNING WITH HUMAN SUBJECTS

Location of Extra Stimuli	Pattern	Study	N	Mean Errors	Adjusted for Differences in Groups	Theoretical Score
Bell-right, no shock	A	(M)	26	116.1	116.1	−12
	A	(C)	47	103.8	120.4	−12
	A	(H & K)	26	58.2	111.4	−12
	B	(M)	26	125.5	125.5	−12
	B	(S & L)	25	64.7	109.9	−12
Bell-right, shock-right	A	(M)	26	118.0	118.0	−9
	A	(C)	34	105.7	122.3	−9
Bell-right, shock-wrong	A	(M)	26	126.3	126.3	+2
Bell-right, shock-wrong, no signal wrong		(MacPhee)	36	133.6	126.0	+2
Bell-wrong, no shock	A	(M)	26	145.2	145.2	+4
	A	(C)	25	108.6	125.2	+4
	A	(S & L)	25	115.5	160.7	+4
	B	(M)	26	124.6	124.6	+4
	B	(C)	46	117.2	133.8	+4
	B	(H & K)	26	76.0	129.2	+4
Bell-wrong, shock-right	B	(M)	26	134.3	134.3	+7
Bell-wrong, shock-right, no signal wrong		(MacPhee)	36	140.2	132.6	+7
Bell-wrong, shock-wrong	B	(M)	26	143.4	143.4	+18
	B	(C)	34	128.8	145.4	+18
Bell-wrong, shock-wrong, no signal right		(MacPhee)	36	153.1	145.5	+18

The studies referred to are as follows: (**10, 14, 8, 12, 9**).

pletion of the learning. Some subjects may especially desire to please the experimenter, others to 'show off,' others to gain prestige with fellow students, or to return as quickly as possible to their own interrupted activities, etc. Muenzinger has suggested that with adult subjects there may be such a strong motivation to learn the problem that the extra stimuli could not significantly affect the learning scores. It may be that studies that are not in line with the trend shown in Table II result from differences in emphasis on these general motives.

Though we have considered the rôle of these extraneous stimuli both as related to the learning goal and as isolated stimuli, we would not imply by the latter term that responses are not motivated. We are rather emphasizing that the complex of motives in the background of the behavior stream is in a constant state of flux, with motives both reinforcing and competing with each other. The dominance of a learning motive may be overcome by non-learning motives. And to the degree that the responses are reinforced by 'satisfaction' of whatever motive is acting, to that degree there is a tendency for the repetition of such responses, whether helpful or not for the learning decreed by the experimenter. The law of effect, as a statement of the rôle of 'satisfaction' for an active motive, is not contradicted by the studies using extraneous stimuli. The law of effect must, however, be interpreted in terms of the motive dominant at the moment of response.

It would appear, however, that the

repetition of any responses, whether made under motives leading to learning or not, is also subject to the distracting, disrupting, awakening, or marking effects of any stimuli within the field of the organism. Some of the extraneous stimuli would likewise have an independent motivating effect; some would not. Shock has a motivating effect to escape, and it has a disrupting, awakening, and probably marking effect. We have assumed that the auditory stimulus used in the experiments with the punch-board maze had likewise a motivating effect, though much less than shock, as well as being disrupting, awakening, and also probably emphasizing. If, however, a use had been made of a slight increase in illumination following a response, such a stimulus might not have an independent motivating effect with human subjects though it might be awakening, emphasizing or possibly disturbing in its effect on the repetition of responses.

In the usual experimental situation extraneous stimuli are kept at a minimum, and even when present with their own motivating effect, the learning motives finally dominate. Both rats and man will learn in situations where they must take the path which leads over painful stimuli as shock. The dominance of the learning motive indicates that it is stronger than the escape motive. Why and when the learning goal takes precedence is difficult to determine. The laws governing the continual flux in the motivating background of behavior are practically unknown. It is possible that oscillation, to which Hull attributes variability of behavior, may be a reflection of this flux of motives.

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REDUCTIONISM

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I

As more and more remarkable discoveries have been made in the various fields of science during the past half century, scientists and philosophers have found it necessary to formulate theories that represent a radical break with the classical theories of the previous three centuries. Among these newer developments may be mentioned the following: in the field of physics the revolutionary theories that grew out of the work on the electro-magnetic field, radio-activity, the photo-electric effect, etc.; in mathematics the theories of relativity with their radical reinterpretation of time, space, and causation; in biology theories of organicism and emergent evolution; in psychology and social psychology Gestalt theory, organicism, and field theory; and in philosophy such theories as dialectical materialism and holism.

It is not my intention to discuss any of these new theories in detail. What I wish to call attention to is that nearly all of them involve certain common basic principles and that this would suggest that these various theories represent, in broad outline, a new metaphysics of science that is replacing the metaphysics of mechanistic materialism which dominated scientific thought during the seventeenth, eighteenth, and nineteenth centuries. If this is true, then it would seem that those biologists, psychologists, and social scientists who still continue to dismiss these newer theories, when they are employed in their fields, by contemptuously calling them 'vitalism,' 'organic analogies,' or 'mysticism' do so at considerable risk.

II

The two fundamental principles underlying these theories are the principle of the whole and the principle of levels. The first is embodied in all of them; the second in many of them. Acceptance or rejection of these two principles means a radical difference in a thinker's approach to and interpretation of the phenomena of his special field. The mechanist, of course, rejects them, and, thus, in the view of all those who accept them, is guilty of interpretations that are both anachronistic and superficial and involve a number of serious fallacies.

The principle of the whole has been expressed in various ways. Wheeler's formulation will do as well as any. His law of field properties states that "wholes exist in their own right over and above the parts or ingredients" and his law of determined activity states that "the whole conditions the activities of its parts" (18, p. 26). Field theorists would say more simply: There is no local determination. There are many different types of wholes in nature: atoms, the solar system, amoebae, cells, a human being, a clan, a nation, etc. Each whole has qualities that are over and above the parts or elements of which it is composed, and the whole must be treated as a unit and cannot be accounted for by a mere analysis of the units of which it is composed. Thus a social group has qualities not possessed by the individuals that constitute it. A whole, in short, is more than the sum of its parts.

One criticism of this principle is that if one held consistently to the principle, it would immediately rule out all analy-

sis, for to analyze is to break down a whole into its constituent parts; yet only by such analysis can one begin to understand a complex whole and only by discovering the simpler units of such a whole can one explain it adequately. Without analysis we can only stand mute before a given whole and mystically contemplate its ineffable wholeness.

To this criticism, the defenders of the principle reply by agreeing that analysis is, of course, necessary, but that it represents only one half of the process of thought. Analysis must be constantly supplemented by a synthesis which takes account of the dynamic interactions of the parts and the determination of the parts by the functioning whole. Too much scientific thinking of the past has overemphasized analysis to such an extreme degree that the present emphasis on the whole constitutes a long-needed corrective to this one-sided approach to phenomena. Coghill's classic monograph on *Amblystoma* reveals the most painstaking analysis, although the main thesis of the work is the holistic principle that "Behaviour develops from the beginning through the progressive expansion of a perfectly integrated total pattern and the individuation within it of partial patterns which acquire various degrees of discreteness" (5, p. 38).

Another criticism is that while the principle of the whole may have some validity in biology, the attempt to use it in such fields as psychology and sociology is nothing but analogical thinking. Certain early schools of sociology and political science, which leaned heavily on the new science of biology, and historical systems like that of Spengler deserve this criticism, but the principle of the whole, while many biologists have accepted it, does not stand or fall with organic phenomena. No one, I think, would care to maintain that Einstein's general theory of relativity, *Toward a Unified Field Theory,* is based on biological analogy.

A third criticism is that the principle smacks of mysticism or vitalism and represents an unscientific attitude of certain biologists, psychologists, and social scientists. But it is not so simple as that. The principle holds for mathematical and physical aggregates as well as for organic and social aggregates. As the mathematician Hyman Levy points out:

> "One of the most urgent problems of modern physics is this problem of synthesis. If a series of particles, independent and self-subsisting, molecules, atoms or electrons, be brought together, how can the theorist account for the fact that the group *as a group* is something more than a mere set of independent particles? As a group it possesses its own characteristics, just as a battalion of soldiers is something very different from the same number of isolated and separately equipped individuals. Group activity is a real thing, showing its own special laws of growth, change and decay, and it is not necessarily to be expressed entirely in terms of the characteristics of the separate components when seen in isolation" (13, pp. 8–9).

The second principle, the principle of levels, is a corollary of the principle of the whole. According to this principle, there are in the universe various stages or levels of phenomena characterized by increasing complexity, interdependence of parts, and unity. The subatomic, the atomic, the molecular, the organic, and the super-organic are examples of these stages or levels. Emergent evolutionists maintain that as evolution has proceeded throughout nature, life and consciousness are qualities that have 'emerged' as matter has evolved from simple to more complex forms. New wholes with new and unpredictable qualities have arisen out of the coming together of elements that previously possessed none of the qualities their

new combination gave rise to. Thus the emergence of life from inanimate matter is, if still a great mystery, no greater a mystery, theoretically, than the remarkable qualitative differences displayed by the hydro-carbon series. Gerard emphasizes the qualitative differences that appear in organisms as we go up the scale of life:

"The impulse which races along a nerve or through the labyrinthine pathways of a brain is merely an improved model of the electro chemical change which crawls along the surface of any mass of protoplasm. Yet the advent of a nervous system and its steady development have conferred seemingly new qualities upon its possessors.

"It is a far cry from the tropistic bending of plants and the solgel performance of an excited amoeba, through the convulsive generalized collapse of a disturbed sea anemone, to the perfectly coordinated running movements of a stimulated dog. . . .

"But it is perhaps an even greater jump from the dog without a cerebrum to one with it, or to man, than from the amoeba to the decerebrate mammal. For, with the cerebral cortex, there clearly emerge qualities of behavior which were only dimly foreshadowed in more primitive animals—the qualities of modifiability, of learning, of choice, and ultimately those of analyzed awareness, of abstract and symbolic reasoning, and of prescience. Somehow, the few ounces of additional nerve cells built into the gray shell of the human brain have pointed up the potentialities of voluntary and rational behavior to an unprecedented level.

"Pointed up, but not created. There is no more a sharp demarcation between thoughtful and automatic acts, between conscious and insensate states, than there is between living and inanimate systems. When complexity at one level of organization becomes sufficient, the new permutations are so great that there seems to be a qualitative discontinuity, a new emergence" (6, pp. 418–419).

Newman takes a similar position:

"The significance of this series of levels of life units is not far to seek. Each level of unity differs in its properties from the level below and above it. Each has properties of the whole that are different from the additive properties of the parts. These new properties that emerge as the result of a new level of organization are not to be regarded as mystical or 'vital' properties in the sense that they are immaterial or supernatural. They are the natural properties of matter at the particular level of organized unity in which they are found. Yet these properties of new wholes do not seem to be fully accounted for on the basis of separate physio-chemical properties of the component units as they are now known to us. This admission may seem to some extreme mechanists as a concession to vitalism. To us it seems to be a conception that may save the mechanistic view from complete stultification" (15, p. 214).

Whether the emergent evolutionists are right or wrong in their explanation of the origin of life from inanimate matter is irrelevant to this discussion. The important point is that emergent evolutionists, organicists, holists, Gestaltists, dialectical materialists, and field theorists all agree in stressing in some respect the principle of levels, that there is a hierarchy in nature, however one may explain its origin, and that attempts to interpret a higher level of development in terms of a lower level is at best a clumsy makeshift and at worst a system of specious metaphors based on unconscious analogy.

Bridgman, who certainly cannot be charged with organic vitalism or mysticism, questions whether the large can always be analyzed into smaller units and warns against being confused by models (1, pp. 51, 53). He also points out that in electrical energy, unlike two-dimensional geometry, the sum is more than or different from its parts and that new effects may appear in the aggregate (1, pp. 220–221). "It seems fairly evident then," he writes, "that the laws of nature cannot be reduced to either

those of mechanics or of electricity, nor probably, as is suggested by quantum phenomena, to a combination of both" (1, p. 200).

The biologist Haldane ridicules the metaphorical thinking involved in such terms as 'living matter' and 'physiological mechanisms,' and maintains that the use of the word 'mechanism' in biology "is a mere empty formality" (10, p. 11). He argues further that nothing but confusion can arise when attempts are made to reduce biological or psychological phenomena to physics or chemistry or psychological phenomena to biology. Such attempts involve, in philosophical terms, a confusion of categories (10, p. 89). Kurt Goldstein is in a complete agreement with Haldane on this question. He declares:

"A chemical description will never adequately explain a biological process. It can never do more than disclose factors—essential ones, we grant you—necessary to the course of the performances, and can only show how they appear under isolated conditions. To understand the phenomena, and by means of these, to understand the organism, requires above all *behavior analysis*. What we call 'chemism' will certainly play a part in this analysis, more important than we sufficiently realize today. But I believe that what we have learned from analyzing reflexes, applies equally to all knowledge gained through similar methods. And what we have learned is that the phenomena observed, in isolating investigations, have special properties, from which it would be impossible to draw any valid inferences about the real nature of the organism" (7, p. 208).

And later in the same work he makes the italicized statement: *"Organic nature cannot be understood with the tools of mathematical, natural science"* (7, p. 409). Haldane takes the same position (10, pp. 14–15).

III

If those thinkers who accept these two principles of the whole and of levels are right, then those who, like mechanistic materialists, do not observe them in their thinking are guilty of a number of errors or fallacies in their approach to and interpretation of data. All of them are sub-types of a general fallacy which I propose to call *reductionism*. Reductionism may be defined as *the attempt to explain a complex interrelated whole in terms of its simpler elements or parts or in terms of elements belonging to a lower level of phenomena*.

It becomes increasingly apparent that one of the strangest delusions of scientists has been the conviction that, to be scientifically sound, one order of phenomena can and should be reduced to another. Psychology, it has long been felt, becomes scientific in so far as it is reduced to physiology, neurology, biochemistry, or physics. As a matter of fact, even in the physical sciences, which are the most firmly established, there is little evidence to support such a view; most of the important developments in chemistry have gone on quite independently of developments in physics. The same is true in biology. Although many biologists have attempted to reduce biology to mechanistic materialism, all the important theories and laws of biology have been formulated quite independently of physics and chemistry. If one did not understand the power of traditional thought-ways, it would be laughable to note how a number of psychologists, in order to justify their science, have in recent years seized upon the distinction between molar and molecular phenomena. They too can be scientific without having to reduce psychology to the molecular level! This is a tardy recognition of what should have been apparent all along. Nothing, I

think, illustrates more clearly than this the hypnotic spell that reductionism still casts over the scientific mind.

The fallacy of reductionism assumes many different forms, of which the following are examples:

(1) *The Fallacy of Isolation.*—This fallacy consists in isolating a part from a whole and studying its structure or function without taking into sufficient consideration its relations with and dependence on the whole of which it is a part. Much of the work on the cell in the field of biology, the reflex in psychology, and the leader in sociology and history has been vitiated by this kind of approach. The reflex, as I shall show later, is nothing but a pure heuristic fiction; so also are the 'economic man' of classical economics and 'the Crusoe and his man Friday' of economics and political science. Goldstein in his book, *The Organism,* has especially attacked this fallacy.

(2) *The Fallacy of Mathematical Summation.*—The fallacy consists in thinking that because in mathematics a whole is the sum of its parts, it is also true in nature; whereas, as we have seen, a whole is greater than the sum of its parts. Thinkers who are strong on analysis and classification have been prone to this fallacy. They believe that if one can analyze a whole into its elements or parts, one has adequately dealt with the whole. They fail to perceive that to anatomize a man is not to reveal a human being, but to expose a dismembered corpse. It might be called the scholastic fallacy, for all those thinkers under the sway of Aristotelian-Thomistic thought have been prone to it. Faculty psychology is a product of such thought.

(3) *The Fallacy of Constants.*—This fallacy involves thinking in static rather than dynamic terms. Nature or society is conceived as a conglomeration of fixed units, atoms, or individuals. All change involves a mere reshuffling of the units, which remain constant. Nothing really new, nothing novel, can arise in nature, human nature, or society. Those guilty of this fallacy do not perceive that quantitative additions may suddenly give rise to remarkable qualitative changes or emergents. They hold to the old saying, *Natura non facit saltum.* Psychologists, sociologists, and historians have been prone to this fallacy. There is nothing new under the sun. Human nature does not change. History is a cycle. *Plus ça change, plus c'est la même chose.* Revolutions achieve no lasting modifications of the social structure, for new rulers and new classes take the place of the old. Capitalism is as old as the practice of barter. Eternal recurrence is a law of the universe. Spengler's *Decline of the West,* which is usually considered an organic interpretation of history, is really a mechanistic one, for it is based on the conception of the recurrence of certain fixed patterns or constants.

(4) *The Fallacy of Origin.*—According to this fallacy, an effect can not differ from its cause. Since man by origin is an animal, he is still essentially an animal; and the 'laws' of learning, for example, are the same for animals and men. Since the struggle for existence and the survival of the fittest are laws of the jungle, they are also laws of human society. Much of the popular resistance to the theory of evolution has been due to implicitly thinking of evolution in terms of this fallacy. Likewise the resistance to Freud's theories about the sexual origin of 'higher' activities like art and religion. Another example is the view that since religion arose out of ignorance and superstition, it is still a form of ignorance and superstition. Many scientific writers by their own implicit acceptance of the fallacy have unintentionally and unnecessarily prejudiced people against many

important scientific theories. The layman's refusal to believe that he is just an ape is a sounder position than the biologist's insistence that he is.

(5) *The Fallacy of Metaphor or Analogy.*—This fallacy consists in using words intended for one level of phenomena at some other level. Man can never completely escape this fallacy, since he cannot help thinking anthropomorphically about all non-human levels, whether it be the physical idea of force or the supernatural idea of God. But he can avoid the careless mixing of terms intended for different orders of phenomena. One of the commonest illustrations is the statement, 'Man is a machine.'[1] Only those biologists and psychologists who can ignore etymology, the standard definitions of an unabridged dictionary, and traditional connotation can be satisfied with such definitions. It is one thing to say that man has some of the characteristics of a machine; it is another to say, 'Man is a machine.' Another illustration is the misuse of purpose by psychologists. Purpose, by definition, is a mental act of setting before oneself an object to be obtained. To discuss 'purpose in nature' or 'purpose in animals' is nothing but careless metaphorical or analogical thinking. Plants and animals have ends; they do not 'purpose' or 'propose.' Semantics should be a required course for all students of science.

(6) *The Fallacy of Models.*—This fallacy consists in interpreting complex phenomena by models and then being misled by them. Eddington points out that one of the great differences between nineteenth and twentieth century scientists is the former's faith in models and the latter's dislike of them. The use of models is a kind of analogical or metaphorical thinking, and they misrepresent as much as they represent. Picturing the atom as a sub-microscopic solar system is one example. Comparing the nervous system to a telephone system is another. Most of the current theories on habit formation and learning involve a mixture of analogical thinking and over-simplified models rather than definite neurological knowledge, though too few psychologists are aware of the fact, just as their predecessors did not realize that their theory of faculty development through disciplined effort was largely based on the model of strengthening muscles by exercise.

[1] Compare Clark Hull's "Organisms are more or less successfully self-maintaining mechanisms. In the present context *a mechanism is defined as a physical aggregate whose behavior occurs under ascertainable conditions according to definitely statable rules or laws*" (11, p. 384). 'Self-maintaining mechanism' is something to conjure with! 'Mechanism' is here defined so broadly as to be practically meaningless.

IV

Psychologists and social scientists have been most guilty of reductionism. Late-comers in the field of science, they have been unable to develop independently their own special methods, techniques, concepts, and theories.

Indeed, the history of modern psychology from John Locke to John B. Watson has been marked by a series of failures to achieve a scientific psychology that would prove superior to the Aristotelian-Thomistic psychology of the scholastics. The chief cause for this failure, it seems to me, has been the persistent attempt to interpret psychological phenomena in terms borrowed from either biology, physiology, or mechanistic materialism and as a logical corollary of this attempt, the ingrained, unquestioned assumption that this could be accomplished if only the mind could be analyzed into its simple, irreducible elements out of which it was composed. In short, the fallacy of reductionism in its various forms.

Locke developed his theory of simple

sensations, out of which all mental life is built, and this approach was the model that later psychologists have followed until very recently. Hartley attempted to achieve by means of his law of association a complete psychic atomism; Condillac in France made a similar attempt to reduce mental life to an aggregate of atoms of sensation, and so also, later in England, did James Mill. Mention should also be made of the faculty psychology and its later development in Gall's phrenology. The development of experimental psychology in Germany turned psychology into a special branch of physiology, and Wundt found the elements of psychology to be sensations, images, and feelings.

In more recent psychology three of the most important concepts are good illustrations of the reductionist fallacy and of the falsification of reality that ensues from such approaches to mental phenomena. These are 'instinct,' 'drive,' and 'reflex' and its modification, the conditioned reflex or response.

The first of these need not detain us, as most psychologists now perceive the inadequacy of the concept. The best that can be said for the instinct psychology is that it was a much-needed corrective to the over-intellectualized psychology that up to then was in vogue; the irrational, non-conscious side of man's mental life received the emphasis that it deserved. The worst that can be said is that 'instinct' was but a name for a number of mysteries of animal and human behavior, which still went unexplained. One could endlessly multiply the number of animal or human instincts. There was not only a sexual instinct, but a maternal instinct. Just why no one added—perhaps some did—a paternal instinct, a fraternal instinct, and a sororal instinct is not clear. Certainly Freud had the right to conclude that if there is a survival instinct, there is also a death instinct. One can read only with wonder now McDougall's chapter, 'The Principle Instincts of Man,' in his influential book *Social Psychology*, which went into fourteen or more editions.

'Drives' have now succeeded 'instincts,' but the improvement is open to question. 'Drives' are subject to the same criticism as instincts. They are attempts to establish certain definite basic patterns or tendencies that remain constant, or fairly so, amid all the complexities and transformations that arise as various kinds of organisms undergo development and change in response to changing environmental conditions. To say that the act of an animal devouring its prey is 'fundamentally' the same as that of a modern man eating his dinner in the bosom of his family, that both are manifestations of the hunger drive, is to be guilty of oversimplification, if not of outright falsification. One can argue just as cogently that they are 'fundamentally' different acts, and the right kind of psychology will forego such facile atomic reductionism for a more accurate interpretation of total situations, such as field theory at least attempts. The list of drives like the list of instincts differs with different writers. What is worse, primarily physiological activities like 'hunger' and primarily psychological activities like the 'aesthetic drive' (!) are lumped together in one category. Goldstein has concluded that there is no reason to assume the existence of special drives; they are special reactions in special situations (8, p. 140 ff.). What Brown, writing as a field theorist, has to say in criticism of instincts, applies also to drives: underlying all the attempts of the instinct theorists "is a common methodological weakness, namely, the desire to find certain human reaction tendencies which remain constant, in-

dependent of changes in field structure" (2, p. 91).

In recent years the inadequacy of 'reflexology' has been stressed by a number of psychologists belonging to the organic, Gestalt, and field theory schools.[2] It has been shown that the reflex is an artificial construct of the human mind, and one in which the fallacies of isolation and reduction have done yeoman service. Only in highly artificial situations, such as those obtained by Pavlov in his experiments with dogs, in which the modifying influences of the total organism and the environment are ignored, can the pure reflex be demonstrated, if at all. Blinded, I venture to say, by the reductionist fallacy too few psychologists have noted Watson's significant admission in the book which became for over a decade the bible of American behaviorists:

> "It [the reflex] is an abstraction because reflex action in the eye, the leg, hand or foot can never take place in isolation. . . . The term reflex, however, is an extremely convenient one and by it we mean the simplest type of activity that can ordinarily be produced. Theoretically we might have a pure reflex . . ." (17, p. 264).

As a heuristic fiction no one can quarrel with the 'reflex' except on the ground that it is an inadequate hypothesis to account for the complex interrelations of organisms and environments; but as an actual, basic, innate unit of human or animal behavior, no one at all acquainted with the philosophy of science can accept it.

If, then, the reflex, by Watson's own admission, is but a convenient term, an abstraction, based upon the fallacy of isolation, what can be said of his and other attempts to explain the development of language and the higher intellectual activities on the basis of reflexes and their conditioning? I venture to predict that the time is not far distant, if it is not already here, when Watson's exposition of the development of language and all similar ones patterned after his, will seem as over-simplified, artificial, and fantastic as Hartley's associationist psychology seems today—not, in short, good science, but crude metaphysics.[3]

The behaviorism of Watson has now been superseded by other behaviorist psychologies. McDougall has divided behaviorists into 'strict behaviorists,' 'near behaviorists,' and 'purposive behaviorists' (14). The latter, of course, wish to stress behavior while divorcing themselves from the more mechanistic atomism of earlier behaviorism. As I mentioned before, they have come to appreciate the distinction, especially stressed by organicists and gestaltists, between molecular and molar phenomena. Though this is a tardy recognition of what should have been recognized earlier, it is a sign of the new outlook. It is also a recognition, perhaps not too clearly discerned by some, that there are levels of phenomena, and each level demands a different approach and a different type of interpretation.

'Instinct,' 'drive,' and 'reflex' are only three examples of reductionist thinking in psychology. If space permitted, many other, and in many respects more significant, examples could be cited. But the brief allusions to the problems of habit and learning and to comparative psychology in the sections dealing with the various kinds of reductionist fallacies will have to suffice. However, I must add that most of the endless quarrels over heredity versus environment, nature versus nurture, the constancy versus the inconstancy of the

[2] For a summary of the evidence see (8).

[3] See, for example, J. Stanley Gray's account (9, Chap. 5), in which he makes the amazing declaration: "The fundamental laws of physics will explain language and all other forms of organic behavior" (p. 142).

I.Q., the mind versus the body, introspection versus objectivity, *etc.* have been created largely by reductionist thinking. Jennings is attacking this kind of thinking, though perhaps not too clearly, when he writes regarding one of these controversies, that over heredity and environment:

"This confusion is worse confounded by the strange and strong propensity of workers in heredity to flout and deny and despise the observations of the workers in environmental action; the equally strange and strong propensity of students of environmental effects to flout and deny and despise the work on inheritance. If one accepts the affirmative results of both sets, untroubled by their negations, untroubled by definitions that have come from the past, there results a simple, consistent and useful body of knowledge; though with less pretentious claims than are set forth by either single set" (12, p. 206).

The fetish of number and measure among psychologists, it is hoped, will grow weaker; and they will eventually achieve a psychology less dependent upon physics, chemistry, physiology, neurology, or experiments with animals —in other words, a true science of the human mind. The ancients thought that mind or *nous* was a universal substance; modern laboratory psychologists have looked for the mind in the brain and the nervous system; tomorrow's psychologists, I believe, will discover that *the mind is in society* and that so-called 'social psychology' *is* psychology.[4]

V

I have already commented on the persistence of reductionism among psychologists and sociologists. It remains to attempt a brief explanation for this remarkable example of cultural lag. For one thing, there is the prestige of physical science built up through three centuries of brilliant achievements to such a degree that in many minds the only true science is physical science. For another thing, scientific abstractions reflect the existing social relationships. Scientific preoccupation with separate, isolated units or elements of phenomena is a characteristic thought-way of bourgeois society and its whole ideological super-structure in which the emphasis has been on the social and political atom, the individual. Societies, so it was thought, are composed of free individuals who make social contracts and thus form a state. Liberal economists thought in terms of abstractions like the 'economic man'; psychologists studied the isolated individual. Indeed, reductionism in its various forms runs all through liberal thought; and it is not surprising that Herbert Hoover, one of the last of the old political liberals, should supply in his book, *The Problems of Lasting Peace*, illustration after illustration of reductionist thinking.[5]

Tradition and social habit are powerful forces even with scientifically trained minds. A long established theory can be replaced with difficulty despite increasing empirical data to the contrary. Goldstein maintains, and I think cor-

[4] Sidney L. Pressey is one of the more orthodox psychologists who are veering in this direction. In his preface to a new type of textbook of psychology, in which the emphasis is upon social adjustment, he writes: "The text books of a few years ago drew heavily from neurology and physiology and stress was upon physiological mechanisms. . . . Increasingly (the writer believes) psychology will find in these last subjects [the social sciences] matter helpful to it, and will stress what might be called psycho-sociological rather than psycho-neurological problems. Is it not here significant that Terman's most recent book deals with marriage, and Thorndike's latest volume with cities! Might it not be said that forty years ago psychology dealt with mental states, twenty years ago with reflex patterns; now increasingly it is dealing with persons . . . and therefore stressing socio-economic and cultural environment and problems of adjustment thereto . . ." (16, p. xxxi).

[5] For a discussion of the logic of liberal thought see 4, pp. 250 ff.

rectly, that it is actually mechanistic atomists that are theory-bound, whereas the holists and organicists are attempting to refashion their theories to fit empirical fact (7, pp. 514–515).

Another reason for the persistence of reductionism is a matter of temperament and attitude. Many scientists pride themselves on their hard-headed objectivity and realism. The old historical controversy between science and theology still lives in their nervous systems, whether they are conscious of it or not. They are rationalists and mechanists and pride themselves on being emancipated from vain theorizing —from all philosophy, metaphysics, and religion. The irony is that they scorn metaphysics while remaining in the grip of an outmoded metaphysics that they have never cared to examine. Commenting on biologists who turn, like moths to a candle, to physico-chemical explanations, Haldane writes:

"They never realize that they are themselves in the grip of metaphysics, and bad metaphysics, when they endeavour to twist biological observation into the form of physical and chemical interpretation. The manner in which, since the time of Galileo, Descartes, and Newton, European culture got into the grip of bad metaphysics when men supposed that they were freeing themselves from metaphysics and going back to facts is something which future generations will laugh over; and particularly over ideas at present current as to what science or 'exact science' is" (10, pp. 147–148).

In his important work, *The Metaphysical Foundations of Modern Science*, Burtt argues that rationalization has played just as important a part in the acceptance of mechanistic materialism as it played in the acceptance of medieval supernaturalism (3, pp. 302–303). His work is one of many which have appeared in the last quarter century and which look with a critical eye at the unexamined metaphysics of seventeenth, eighteenth, and nineteenth century science. Only the revolution that has taken place in twentieth century physics itself has made this reëxamination possible. Today, thanks to the work of a host of great scientists from Röntgen to Einstein, we know that nineteenth century mechanistic materialism, built so confidently on the foundation of Newton, is inadequate for physics itself, let alone for biological, psychological, and social phenomena.

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SOCIAL SITUATION, SOCIAL BEHAVIOR, SOCIAL GROUP

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Any satisfactory frame of reference, and any systematic analysis of social phenomena, must distinguish among and articulate to one another the concepts of social situation, social behavior and social group. The terms themselves, although possessing a very wide currency, are not as indispensable to sociological composition and exposition as the ideas they designate are to systematic sociological thought. As is true of so many sociological terms, however, their relationships to one another have remained to some extent vague and ambiguous.

Social situation has received somewhat less attention in sociological literature than have the other two terms. In broadest meaning *a situation is a set of circumstances in which some object is located and/or in which some event is occurring.*

The object may be inanimate, under the influence of inanimate forces, or it may be animate and a subject conscious of itself and/or other objects. In either case a situation is a set of circumstances, rather than an object alone.

Situation is thus a relational term, which means that it implies a plurality of items bearing some sort of relationship to one another. These items may be of any distinguishable kind involving location, substance, movement, or change as they relate to one or more other items.

Since a situation involves relationships among items, it is concerned with the whole-part question. By definition one item, object or event is not a situation, because one item is not relational. Nor is one circumstance a situation. One item can be only a part of a relational entity and one circumstance can only be a part of a set of circumstances. And neither one item nor one circumstance constitutes a whole situation, for a part cannot be a whole.

On the other hand, two unrelated items, or two unrelated circumstances or one item plus one circumstance unrelated to it, do not constitute a situation. Significant relationship between parts is necessary. The addition of unrelated parts does not make up a whole, because each whole depends on a non-additive relationship. This relationship is one of function, interdependence, mutual influence. The item depends on the circumstance. The circumstance also influences the object. Only where such relationships exist do situations exist.

The concept of situation presented here is simple enough but it has important implications. Thus, it is not possible to reconcile the present view with the common usage of situation as synonymous with environment, since the environment is merely the set of circumstances surrounding an item. Situation as defined here is synonymous with item-in-environment. The item is not in a situation conceived of as something separable from the item. The item is an inseparable part of the situation. When the item becomes unrelated to the circumstances (environment) the situation has ceased to exist. It therefore becomes necessary to avoid thinking of the situation as having attributes that are not partaken of by the related item-in-circumstances (-environment). A situation has attributes as a whole. The item in it may have certain attributes that are distinguishable

from the attributes of the circumstances but the situation's general attributes are always relational. For example, a situation may be described as one of struggle against nature when a person is opposing the physical forces of the environment. The person and the environment are easily distinguished. But neither alone has the attribute of struggle of the person against physical force.

Again, the situation may be one of social conflict where people are opposed to other people. For example, management and labor groups may have broken off negotiations and may have resorted to violence with reference to each other. People in such circumstances of violent and insoluble disagreement make up a situation of social or group conflict or crisis. The situation involving these same people in the same physical environment may be one of celebration, peaceful production, discussion, or active rioting, depending on their psychological states and their interaction. All of these situations involving management and labor groups belong to the general class of social interaction situations in which the environment is partly non-social and partly social, in which two or more people are concerned, and in which the relationships between the people are more important than the relationships of people with the non-social environment.

In view of what has been said in the preceding paragraphs, *a social situation* may be defined as *a set of circumstances in which an agent is located in relation to a social environment, or is engaging in social activity* (5). The social situation is but one variety of situation, and, as such, has two basic qualities (plural circumstances perceived as related to each other, and location of item in such circumstances) common to all situations. On the other hand, the social situation has the unique quality of association not found in non-social situations. This association consists of one agent [1] influencing or being influenced by a social environment. This social environment, in turn, may be another agent-in-environment whole (or plural agents-in-environment), an artifact remaining as a result of past social action (2), or a combination of another agent-in-environment whole with an artifact.

It should be noted that this definition of social situation allows for potential social behavior as well as occurring social behavior. Potential and actual occurring social situations may be thought of as two orders of social situations. The former is a situation in which social action may take place, but has not begun, while in the latter situation social behavior is proceeding. Preliminary to the potential social situation is, of course, a situation not yet become potentially social, because the temporal and spatial relationships necessary for social situation and social response do not exist (5).[2]

It is in a psychological analysis of the potential and dynamic social situation that the most complete understanding of the relationship between social situation and social behavior may be obtained. Social behavior may best be analyzed in terms of stimulus and response of each of two organisms acting

[1] The agent is necessarily a living organism, and for practical purposes an animal with the power of freedom of movement and ability to act to some extent in a non-routinized manner. It is also obvious that the most significant social situations involve human beings.

[2] To these three classes of situations may be added two others deriving from the important distinction between unilateral social action and reciprocal social behavior. When these are added, 5 classes of situations, 4 of them social, are recognizable. These may be further increased by distinguishing between the forms of reciprocal social actions: interaction, unincorporated collective action, and corporate (group) action. A classification is suggested at the end of this article.

as stimulating agents to the other, the resulting stimuli being prerequisite to perception, manipulation and attainment (1, 2).

The social situation can only be potential prior to social action which, in broadest terms, is change involving response of an agent to a social environmental stimulus. In its extreme form, then, the social situation becomes dynamic, behavioral, when social stimulation of an agent begins. And it continues to be dynamic as long as social action continues, that is, until equilibrium of forces in the situation has been reëstablished.

It is, however, an oversimplification of the concept of social behavior situation to stop at this point. Just as a social situation is potential prior to the initiation of a social impulse by means of social stimulation and social response, so the first stimulus (impulse) of one agent in what has been a static situation is, in terms of the fullest kind of interaction between two or more people, merely a preliminary or potential stage in the full unfolding of social behavior. That is to say, any preliminary stage represents only the potentiality of a later stage in a series, not a necessary conclusion to the sequence. Thus stimulation (impulse) is the preliminary stage of potential response of perception of social relationship and significance of the situation and the stimulation. Perception, in turn, is a potential stage for manipulation and attainment, but not necessarily followed by them. In one respect a social situation is not behavioral until manipulation which involves readily observable movements is taking place. However, the most satisfactory conclusion seems to be that all parts of the cycle of activity are equally dynamic, even though not equally observable or equally potent as social stimuli.

We may summarize our position by saying that a situation may be social, without being dynamic, behavioral. Such a situation is purely a matter of location of one or more potential social agents. This sort of situation may become behavioral, at which time one (or more) social agent is influenced by its environment and a behavior sequence is carried out. The social behavior situation is, however, the complete set of circumstances, including agents-in-environment (physical and social), while the social behavior itself consists in the impulse→ perception→ manipulation→ attainment cycles in which the agent (or agents) are engaged. Situation, as was explained before, is always more than agent, more than action, and more than agent-in-action. Expressed in the same terms, the social situation is agent-in-social-environment. One class of such situations is static (quiescent agent located in relation to a static social-environment) and another is dynamic (agent stimulating or responding to a social environment).

The definition of social situation employed here provides for all kinds of social actions, including unilateral as well as reciprocal social behavior of individuals (2), unincorporated collective units and corporate units (4, 6). In the first of these there is only one active agent at any given time, who acts with reference to some artifact, which, as a rule, is capable of functioning as a stimulus to a respondent, but the agent does not receive an immediate return stimulus from any other agent in the environment.

Reciprocal behavior, both in face-to-face circumstances and in indirect contact circumstances, is fully behavioral and fully social, since it involves change of two or more related organisms in which each stimulates and responds to the other or others. The conception of social behavior situation is equally applicable to numbers of individuals act-

ing together as units, as is true of unincorporated collectivities and social groups (4, 6).

However, the complications of collective situations and behavior are so much greater than those of interaction between individuals that they deserve special attention. Furthermore, it is only as a result of such analysis that the relationships between social groups, social situations and social behavior are to be clearly understood.

The chief problems of the relationship between the social group and the social situation are: (1) The nature of the social group, and (2) the corollary question: is the social group always and under all conditions a social situation?

The concept of group is a common one, but with difficulty defined to everyone's satisfaction. It is generally used to signify the existence of a plural number of objects of one kind as a singular object of another kind, the two (or many) as one. Such a conception is broad enough to embrace two distinct ideas. One of these refers to a plural number of independent items (inanimate objects, animals or people) as a group. The second idea is that the items are active organisms, usually human ones, who are bound together by such psychological characteristics as habits, perceptions and attitudes. This conception is the usual one employed in sociology. Accordingly, we may define a *social group* as a *unit consisting of a plural number of separate organisms (agents) who have collective perception of their unity and who have the ability and tendency to act and/or are acting in a unitary manner toward the environment.*

The unity of the group thus has reality in the experiences, habits and attitudes of the members and in the actions of its members with reference to each other and the environment. The most important of these bases of unity is unquestionably perception. Without perception of unity there could be no unity of action, for unity of action necessarily means that each member perceives to some extent what each other is thinking and planning and is ready to relate his own overt behavior as well as perceptions to those of the other members.

It might be questioned whether the group exists when it is not acting. Possibly an extreme behaviorist would take the position that the group exists only when it is engaging in action. This is undoubtedly a safe, conservative position to take. It is matter of fact, but it is also atomistic and of no value in prediction or interpretation of social events. The discussion of the social group in such terms is equivalent to the discussion of behavior without regard to perception, which includes memory, imagination, anticipation or planning. Without such concepts there is no hope of understanding very much of what is actually sensed.

If the question of the group's existence except in action is put on a perceptive level also, then the matter is more delicate. When there is an absence of perceptions of group membership or group existence by one person, so far as that person is concerned at the time the group does not exist in a perceptive sense, and therefore it does not exist in an active sense. This is concluded on the assumption that perception of the group is always present when overt actions of people are group actions. However, even when neither perception of the group nor overt group action is occurring, the group may still exist in a dormant, potential sense, in the existence of habit systems, attitudes and other tendencies which may be aroused by a suitable set of stimuli. Social group as potential thus may exist regardless of perception or overt action.

However, the tendency toward group action is not in every case certain to result in group behavior. The existence of a group as action tendency, therefore, does not possess the same degree of reality as does an active group at any of the parts of the cycle of activity (impulse, perception, manipulation, or attainment). Group as potential is only equilibrium preceding the active group which is in a state of disequilibrium.

The question as to whether the social group is always and under all conditions a social situation now has a clear answer. The circumstances will determine whether the social group is a social situation. If the circumstances are those of an active social group, a social situation necessarily exists, since, regardless of whether overt social action is transpiring, when the group is perceived by one person that person is acting on a perceptive level with reference to a social environment. There is, of course, no argument about the existence of a social situation when overt group action is occurring.

When there is not even the perception of a social group, there is no perceptive group situation, although there may still be a perceptive or even overt social situation on a level below that of the social group. When there is no perceptive group situation, nor a perceptive or overt social situation, there still may be a potential or dormant group situation in which one or more persons, not yet actively perceiving a group, exist in a non-perceptive status, as when asleep. Such people may be group members and they may invariably act in group terms when awake, but the group can only be dormant when its members are not acting as members.

The nature and relationship of the various concepts that have been discussed may be summarized briefly in the following classification scheme:

Level of Situation	Phases of Unit of Behavior	Characterization of Situation
1. Potential non-social situation	No behavior occurring	Circumstances suitable for non-social stimulus and non-social response but not for social action
2. Non-social behavior situation	All phases of behavior (impulse, perception, manipulation, attainment) are found	Circumstances where non-social action is occurring
3. Potential unilateral social situation	No behavior occurring	Circumstances suitable for unilateral but not for reciprocal social action
4. Unilateral social behavior situation	All phases are found	Circumstances where unilateral social action is occurring
5. Potential reciprocal social situation (including potential interaction, potential unincorporated collective behavior and potential group behavior)	No behavior occurring	Circumstances suitable for interaction, unincorporated collective behavior and group behavior
6. Reciprocal social behavior situation *a*. Interaction situation *b*. Unincorporated collective behavior situation *c*. Group behavior situation (group in action)	All phases are found	Circumstances where *a*. Interaction occurring *b*. Unincorporated collective behavior occurring *c*. Group behavior occurring

The conclusions of this analysis may now be stated more completely. Social situation has the most extensive reference of the three concepts under consideration. The social group as habit and tendency is a part of one class of potential interaction situation: the class consisting of two or more members of one or more inactive groups. The social group in action is similarly only a part of the class of reciprocal social behavior situations.

Social situation is also more inclusive than social behavior. The unilateral social behavior situation is more than the unilateral social behavior that occurs in it. And the same thing is true of the reciprocal social behavior situation in relation to reciprocal social behavior. Social situations also include potential unilateral and potential reciprocal social situations.

Social behavior, from one point of view, has a more extensive reference than social group, but from another viewpoint has a smaller reference. The social group is never a unilateral phenomenon, either potentially or dynamically, and consequently has a narrower meaning than social behavior, which includes unilateral social actions. On the other hand the social group, as potential, exists on many occasions when reciprocal social behavior is not transpiring. It might also be argued that the potential social group exists so much more frequently than unilateral social behavior, and involves so many more people, that the concept of social group has the more extensive reference of the two concepts.

In general, it is most accurate to conclude that the referends of the three terms dealt with in this article are of different but over-lapping character, and that there is no completely satisfactory answer to the question of the relative extensiveness of their referends. If these essentially relational and qualitative concepts could be satisfactorily converted to a purely quantitative basis, an answer could be obtained. But such a conversion would necessarily violate to an important extent the meanings they possess, and would eliminate the value of the concepts in the analysis of social phenomena.

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ABNORMAL ANIMAL BEHAVIOR AND CONFLICT

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The role of emotional conflict in determining disorganized behavior patterns in the rat is scarcely more striking than the role of the experimenter's own emotion in determining his interpretation of the rat's response. I hope that the present discussion will not further confuse the issue by increasing the significance of personal bias in the consideration of experimental observations.

During the past six years many investigators have reported studies of a disorganized pattern of rat behavior variously termed experimental neurosis (19) or neurotic behavior (15), audiogenic seizure (6, 11, 14, 22, 25, 29, 31), audio-epileptic seizure (30), noise-fright convulsion (32), sonogenic convulsion (23), sound-induced seizure (17). The diagnostic name preferred by a given writer has in general reflected his opinion of the fundamental nature of the behavior. For example, 'experimental neurosis' has been coupled with a clash of incompatible response-tendencies (19); 'audiogenic seizure' and 'sound-induced seizure' are terms adopted by those emphasizing the auditory aspect of the precipitating situation (25, 17); 'noise-fright convulsion' implies a strong emotional component in the complex of antecedent events (32). That agreement concerning the fundamental nature of the pattern has not yet been achieved is illustrated by several statements in a recent REVIEW article by M. E. Bitterman (3). In this theoretical discussion Bitterman defends the position that the convulsive behavior is ". . . a neurotic manifestation stemming from conflict situations to which the animal is subjected . . ." (3, p. 376). This view is presented to support the broader contention that ". . . all situations in which abnormal behavior has been observed in animals may be interpreted as *conflictful* . . ." (3, p. 375).

The last statement demands passing comment, from the point of view of semantics. One wonders what is meant by the phrase "all situations in which abnormal behavior has been observed." Does 'abnormal behavior' mean any behavior deviating from the animal's everyday life? If that is the case, electroshock convulsions (26) and deteriorated behavior following cortical operation (18) must be included in this category—but these are not generally regarded as the result of 'conflict.' Or is a rat's behavior abnormal when it differs from that of the Average Rat exposed to the same current conditions? If so, then *failure* to respond convulsively in certain stimulating situations is the abnormal state, for more than 50 per cent of white rats can be induced to exhibit convulsions (9, 20, 21). Yet such failure to respond has not been demonstrated to result from the simultaneous 'antagonistic adjustments' of conflict. Or is behavior to be considered abnormal when it seemingly fails to obey the principles that govern behavior, *i.e.*, when the scientist cannot satisfactorily predict its course? It would appear more logical to account for such uncertainty of prediction in terms of the incomplete state of our knowledge, rather than invariably to invoke 'conflict.'

It might then be argued that the invoking of 'conflict' as the basis for all abnormal behavior is unjustified, at least until a satisfactory definition of

'abnormal behavior' is forthcoming. But even if the problem of definition is overlooked, as it sometimes is by animal psychopathologists, a brief statement of doubt should be heard for a more specific reason. Bitterman includes in his catalog of abnormal behavior of the rat that disorganized behavior sometimes called audiogenic seizure, and then, in line with the more inclusive argument, points out that this behavior is invariably the consequence of a conflict situation. While this belief may be shared by some other investigators, its acceptance is certainly less unanimous than Bitterman's statement seems to imply. It is my contention that the evidence can be presented in such a way that a contrary conclusion is no less warranted: that this pattern of 'abnormal' behavior need not be interpreted to be the result of conflict.

In the history of comparative psychology many hundreds of experimenters have placed the laboratory rat in thousands of situations, many of them designed to be stressful, some of them insoluble, and a few even painful. According to journal reports, conflict has been induced time and again, with the eagle eye of the investigator alert for overt signs of 'breakdown.' Varying degrees of disorganization have been described; few, however, satisfied the particular observer's criterion of 'experimental neurosis.' This in spite of repeated exposures to drives and counterdrives that beggar enumeration. And then in the period between 1939 and 1944 we find some 75 reports of startling behavior on the part of many members of the rat population. Explosive and disoriented running, epileptiform convulsions, tic-like twitches, and deep coma were the symptoms—extraordinary enough, it appeared to some, to be called neurosis at the very least.[1]

[1] *Cf. Science*, 1939, **89**, p. 93.

The similarity of descriptions of this disorganized pattern emanating from the several laboratories is in contrast to the extremely varied set of conditions evoking the behavior. Analysis of the studies reveals that a great many factors can influence the frequency of occurrence: drugs, age, emotional state, previous exposure to the experimental situation, diet, physical exertion, etc. (10). It appears, for example, that sub-convulsive doses of metrazol (17, 21), 24-hour hunger (8), and vitamin deficiency (27), all increase the likelihood of seizure. But of all the thousands of rats serving as subject in many laboratories and under widely diverse conditions (including 'strong conflict'), only one rat is reported to have given the seizure pattern in the absence of intense sound (13).[2] With this sole exception the invariable antecedent, the most obvious situational constant, has been a loud sound such as the tones of a compressor-driven Galton whistle. Many times conflict seems to have been provided, with a variety of punishing stimuli 'forcing' the animal to respond in a situation for which no appropriate response was available (*e.g.*, 5, 7, 12, 16, 33)—but never a convulsion. Yet many a rat, failing to respond convulsively in a discrimination situation when electric shock, whipping, or in-

[2] To simplify this discussion, the reports of Chick, El Sadr, and Warden (4) and of Patton, Karn, and Longenecker (28) are not considered. They observed convulsive seizures in rats with severe dietary deficiencies, these seizures occurring 'spontaneously' or as the result of certain cutaneous (and perhaps proprioceptive) stimulation. It may be that further investigation along this line will reveal an etiological mechanism more fundamental than acoustic stimulation, one common both to the 'spontaneous' convulsions in the dietary studies and those occurring in response to sound. The finding of such a physiological mechanism, however, would probably decrease rather than increase the significance of 'conflict' in the production of seizures.

tense hunger was the driving force, immediately exhibited seizure behavior when an auditory stimulus was introduced (*cf., e.g.,* 2). Conversely, many rats lacking previous laboratory experience, such as discrimination training, responded with the full seizure pattern after a few seconds of their initial auditory stimulation (1, 2, 9, 23, 24). In other words, every animal exhibiting this 'abnormal' pattern of behavior (with one exception) did so in the immediate presence of an auditory stimulus, while hundreds of seizures were observed in the absence of any deliberately planned conflict and even in spite of the experimenter's diligent efforts to exclude the possibility of such a factor. I find it difficult to interpret these data by saying that all these seizures were neurotic manifestations of conflict, unless 'conflict' and 'neurotic' are to assume new meanings.

That conflict or emotion may play a part in determining an animal's susceptibility to seizure is not denied. It has been reported, for example, that after an emotional state has apparently been induced by exposure to a difficult choice situation, the animal is more prone than usual to respond convulsively when exposed to an auditory stimulus (2). But to term such a convulsion 'neurotic,' and mention only conflict as the causal factor, seems to be straining credulity. The probability of response to sound can also be enhanced by starving the animal during the 24 hours immediately preceding stimulation (8)—but no one has called these convulsions 'hunger convulsions.' There has been no satisfactory demonstration that conflict-induced emotion plays a more significant part in the production of seizures than do several other factors altering the physiological state of the rat. Conflict is thus forced to assume a role secondary to simple exteroceptive stimulation to which the animal is peculiarly sensitive.[3]

In light both of definition and of the nature of the data concerning convulsive behavior in the rat, it therefore seems impossible to accept the statement that ". . . all situations in which abnormal behavior has been observed in animals may be interpreted as *conflictful*. . . ."

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PERSONALISTIC PSYCHOLOGY AS SCIENCE

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Windelband proposed two entirely separate disciplines, one a *nomothetic*, the other an *ideographic* discipline, as G. W. Allport pointed out in quoting this philosopher. The nomothetic disciplines would seek only 'general laws and employ only those procedures admitted by the exact sciences' while the ideographic disciplines would 'endeavor to understand some particular event in nature or society.' [1]

Some interesting questions arise in this connection. We should like to raise some of these questions and offer a point of view. On any conservatism-radicalism scale relating to these questions the writer would doubtless stand well to the left on the conservatism end of the scale.

Whether or not we wish it, the fact remains that the older term *scientia* has come to have a definitely restricted meaning. True enough at first, it had a very broad meaning, signifying simply *knowledge*. Today science, the derived term, means both more and less than the mother term. The term has come to signify knowledge gained through the study of facts established by means of the experimental method. Science has today a body of methodologies which guide the experimenter at every step in his investigation. These are so well known that we need not go further into the matter.

Science, as we know it today, searches for general laws and principles, based upon likenesses and differences in the world of phenomena which is being observed. Physics is not interested in the fact that a particular brick falls from the top of some building and lands upon the ground below. This is but an individual case which illustrates the *law of gravitation*. The chemist is not interested in the fact that my automobile tire bulges out as the day becomes increasingly warmer. He is interested in the *law of expansion of gases* in relation to changes in temperature. And so it is with each of the other sciences.

But we are interested in psychology as science. We are especially interested in the newer personalistic psychology as science. Allport takes a bold stand for the broadening of the concept of science. This may be the proper progressive stand to take but we doubt that our fellow scientists in physics, chemistry, geology, or astronomy will be very receptive to the idea. Perhaps we, as psychologists, could attain a more satisfactory adjustment to the order of things by saying that some of our content or knowledge *is science* while other content or knowledge *is non-science*.

What are the basic criteria of 'scientific content'? What content of a modern-day psychological textbook lives up to these criteria and so is scientific, in the usual sense of the term? To what extent is personalistic psychological content scientific and to what extent non-scientific? What constitutes the scientific content of personalistic psychology? These are some questions which we would now like to discuss briefly.

Speaking strictly, in terms of content or phenomena studied (not in terms of methodology of investigation), we would suggest two basic criteria for truly scientific content or knowledge. First, there is the temporal criterion, the *criterion of durability*. Second,

[1] G. W. Allport, *Personality: a psychological interpretation*. New York: Henry Holt, 1937. P. 22.

there is the *criterion of generality or universality* of phenomena.

In the older sciences, as physics and chemistry, the phenomena or content live up to these two criteria. The law of gravitation expresses a formulation based upon observations of millions of observed phenomena. Objects fell to earth millions of years ago and they do so today. The physicist has great faith that they will continue to fall to earth a million years hence. This content of physics is durable. It stands the test of time. Likewise the law holds not for some one object or locality but for all localities. Here is generality or universality. Thus the basic content of physics is the same whether we live in Africa, Europe, Asia, or a South Sea Island. It was thus a million years ago and will be (presumably) the same a million years hence.

Now let us examine the phenomena of psychology in the light of these two criteria. Let us first consider universality or generality. Some psychologists are chiefly interested in individuals as such. The clinical psychologist and, presumably, most of the personalistic psychologists are more interested in how people differ than in how they are alike. Each Tom, Dick or Mary may become a 'world in itself' and a fascinating and practically important thing to study. But there are others who have a different interest. These psychologists are interested in studying mankind the world over, *trying to find that which is common to every individual.* General laws and principles can and must emerge from such studies. A very recent example of this is Hull's systematic theory of learning. Spearman's G-factor, Thurstone's basic factors, the Weber-Fechner law, and the other theories and laws of psychology represent this interest. The early workers in psychology, Wundt, Müller, Ebbinghaus, Külpe, Titchener, and other outstanding earlier investigators, were devoted to the established scientific doctrine which holds that science deals with the universal. Even the advocates of individual psychology, Binet, Galton, and Cattell, for example, were searching for the *general laws* and *principles* of 'how people differ'! They were not interested in individual uniqueness!

What are the universal phenomena of psychology? What phenomena can be found in all mankind regardless of education and training, regardless of the locality in which the person lives? Since *mass, uncoördinated activity* and *segmental S—R processes* (reflexes) are observed the world over in the newborn, one is tempted to say that these phenomena are general or universal. Then the presence of the *psycho-biological needs, urges, drives* or *tensions* (as those of hunger and thirst), as well as emotional conditions, seems to be universal, common to all humans. Simple, direct *sensory conscious responses,* as sensations of redness, pain, or sweetness, seem to be universal phenomena. Certain *capacities,* as those for learning, remembering, forgetting, recognizing, thinking, and the like, are characteristically human and so well may be considered as universal.

On the other hand such content as ideas, ideals, attitudes, interests, sentiments, purposes, beliefs, idealogies, personality traits (in the Allport sense of the term) and specific habits or motor skills are definitely not common to all people the world over. This obviously is the chief reason for the present terrible war. The American people have different attitudes and beliefs than do the Nazis and Japanese. But even within our own country attitudes, beliefs, and habits will vary from one small locality to another and from individual to individual.

According to the criterion of commonness or universality, then, the second

set of phenomena which we have just mentioned is not scientific. They stand outside the limits of science.

If we apply the criterion of durability we note that we come out with the same grouping and the same statements as those which we have just made in the preceding three paragraphs, a dichotomy of scientific and non-scientific data.

Let us now turn to personalistic psychology. If we look for *that which is common to all mankind*, as did McDougall and Freud, for example, we may emerge with content that is scientific. If, however, we define personalistic psychology as does Allport, each person is a 'unique individual.' Each person is a law and a world unto himself. Here one deals with individual uniqueness and must be concerned with ideals, attitudes, traits and habits. These are not durable or universal in the sense in which we have used these terms. According to the content which Allport stresses, personalistic psychology becomes non-scientific. If one admits both common elements or nomothetic aspects and unique individual phenomena, then personalistic psychology contains both scientific and non-scientific content.

Since G. W. Allport has presented a clear-cut and stimulating *system of personality*, let us look further at his aims and accomplishments. After reading the preface and the first chapter, the reader wonders just what is ahead. The 'generalized mind' psychology is severely criticized and the implication is strong that when a student completes the usual introductory course in psychology he knows next to nothing about human nature, either in himself or in others. Evidently Allport purposes to give the student something that will help him better to understand himself and others!

One wonders if the rest of the book is to be a detailed case-study of some individual whom Allport has chosen as his 'unique world of investigation.' However, such is not at all the case. Allport proceeds to develop *laws* and *general principles* regarding the manner in which personality grows, develops, and functions. Also, he develops principles to aid in the study of individuals.

How does such a course in 'personality' differ from the usual course in psychology? As one goes over the various texts on personality one notes first that they stress the *motivational* aspects of psychology. Second, one notes that they are all influenced by and stress *Gestalt* and so-called *organismic* psychology. Here *molecular* investigation is considered of relatively little importance while *molar* investigation gives the only promise for the understanding of personality. Personality, as a whole, becomes a Super-Gestalt consisting of large coördinated *Gestalten* (as the ego, selves, temperament, and so on). Then there are smaller *Gestalten*, such as traits, in the hierarchy.

In the third place, one notes a stress on *longitudinal* or *developmental* psychology. A text on personality must trace through the typical (universal?) course of development of personalities from the time of conception to the grave.

So, if we combine Gestalt, motivational, and developmental psychology, we have the basic materials for a textbook on personality. But any book of laws and principles of personality must deal with general, common facts. Allport, who so severely criticizes the older scientific psychology which dealt with facts common to all mankind, ends up by *abstracting certain general laws and methodologies!* The only difference between his laws and methodologies and those of a Wundt or a Titchener lies in the fact that his are (presumably) more serviceable in giving self-understanding and understanding of other people. His

laws and principles become as *nomothetic* as any of the older principles of Titchener or Wundt. It is only when one forgets the rest of mankind and *studies one given individual* as a world in himself that one has a true ideographic discipline. Ideographic disciplines cannot be scientific according to the two criteria which we have mentioned.

Psychologists may investigate phenomena which are durable and general or universal. Also, psychologists may be interested in phenomena which are decidedly temporary and local. If a psychologist should test a representative sampling of mankind for, let us say, color deficiency or anomaly, one would emerge with truly scientific data. However, let us suppose that a psychologist went about his community studying the opinion of the people on the merits or demerits of the New Deal and Roosevelt. He would gather in some interesting opinions and beliefs, no doubt, but this *would not be science!* These opinions are local and will undergo change even within the life-span of the people holding said opinions. While the study of attitudes, beliefs, habits and skills may be of immense practical and theoretical importance, such studies are not science in the eyes of our colleagues in physics, chemistry and astronomy. These are the 'shifting sands' of knowledge! As facts they are not common to all mankind and they have not existed as such over the centuries of time. These things constitute the non-scientific aspect of psychology; they are here today and gone tomorrow!

We would like to state one last point of view. There has been considerable dispute as to whether psychology is a biological science or a sociological science. In some colleges and universities psychology is listed as biological science while in others it is classed with the sociological sciences. Some psychologists define their science as a bio-social science. It is right here that we would offer a mild protest. The biological aspect of psychology *is science;* the sociological aspect *is not science.* At least this is true in the usual sense in which the terms sociological and biological are used; here sociological is used to indicate products of the social-cultural environment which, translated into psychological language, means phenomena of learning.

We would prefer to look upon those aspects of psychology which are essentially unlearned or the result of maturational development as the truly scientific content of psychology. These could well be called, as they usually are, biological phenomena. They satisfy our two criteria of scientific data or content. Any effects wrought in man through experience or learning would be unscientific content for psychology because they would not meet the criteria of durability and commonness. Now any system of personalistic psychology, such as that presented by Allport, where the effects of learning are stressed so heavily and where individual uniqueness constitutes the data of study, cannot meet the above-mentioned criteria of scientific data or content. Literally there would be as many separate psychologies as there are individuals, if we carried Allport's doctrine to the extreme! Whenever a new baby is born a new psychology would have to be written! When it died the book would be closed and put aside as of no further interest because this was a unique person! Of course, Allport does not literally mean all this. While traits are unique there is always some *common* aspect present. So the day is saved for the mental tester. Then we have noted that Allport proceeds to develop general principles and laws regarding personality; these may have scientific status.

Of course, one may say that science should be redefined so that it will include phenomena which are individual and temporary. However, it is not so easy to change established denotations of terms. Our colleagues in physics and chemistry might, and probably would, resist any such change in the scientific concept. Again it is a question whether we would desire to change the concept, making it revert backward to its original broad meaning, even if we could overcome tradition. Would it not be better to make another category of knowledge, call it by any other name than science, and leave the basic restricted concept as it is? Why not admit that much of that which is studied in psychology and sociology is simply not science?

When a colleague in physiology or chemistry tells us that our data are not scientific, we become rather upset. When a colleague from the English or the philosophy department tells us the same thing, we are likely to become quite annoyed. We all want to bask in the light of the great Sun-God, Science! But would it not be more broad-minded to be good-natured and admit that much of psychology is outside the realm of science?

One criticism may be made against the criteria of durability and commonness or generality. It may be insisted that these are continuous series, to be regarded as linear continua. Durability is a temporal phenomenon which is at best only a relative thing. And the same for commonness. We admit this criticism of our dichotomizing. But within this framework of relativity we may still, with some arbitrariness no doubt, insist at least on relative durability and commonness.

Science, as we know it today, thinks in terms of millions of years. Our little episodes of life, as wars, economic depressions, national elections, births, and deaths, are but transitory phenomena in a world of geological ages. As such each individual episode is not scientific datum except to the extent that we can find in the episode a universal and durable principle or law. We believe the philosopher Windelband arrived at the proper conclusion, namely, that the knowledge of psychology must be classified into two groups, nomothetic and ideographic. The nomothetic class of knowledge is scientific.

A SUGGESTED IMPROVEMENT IN SEMANTIC USAGE

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During a period of rapid development in a science, many new concepts are invented and old ones given new meaning. There have usually been only two horns to the dilemma posed by the necessity for changing terminology. The first has been to coin new words; the second has been to use extant words, frequently from the lay vocabulary, giving them, however, a new, a more exact, or a more restricted definition. Like many others, I have tried both and have found that neither works well. Neologisms are usually ignored; and, where rigid definition has been given to old words, many psychologists have continued to use these words as they always had before. The result is unnecessary misunderstanding, pseudo-debates and other purely semantic (rather than scientific) battles and confusions.

I wish to offer a third possible alternative which I have tried out in my classes and found to work very well. It is simply to recognize that the same word can be used by different authors in partially or completely different ways. This is done by accepting the words that the various authors have used, appending to them, however, as a subscript, the author's name, which indicates that we are using these words as the particular author used them and in no other way. As my students become familiar with such concepts as 'behaviorism$_{\text{Watson}}$,' or 'behaviorism$_{\text{Hull}}$,' or 'behaviorism$_{\text{Tolman}}$,' I can eventually shorten the subscripts to the authors' initial alone, *i.e.*, 'behaviorism$_{\text{W}}$' or 'behaviorism$_{\text{H}}$' or 'behaviorism$_{\text{T}}$.' A few other concepts for which I have found this procedure useful are field$_{\text{Lewin}}$, abstract$_{\text{Goldstein}}$, emergency$_{\text{Cannon}}$, sex$_{\text{Freud}}$, connection$_{\text{Thorndike}}$, etc. It has also been helpful to speak of behaviorism$_{\text{Gestalt}}$ (behaviorism as the Gestalt psychologists describe it), and Gestalt psychology$_{\text{Behaviorist}}$ (Gestalt psychology as the behaviorist writers describe it). The theoretically minded psychologist will notice here the interesting parallel to the so-called operational definition, or, to be more exact, 'operational definition$_{\text{Bridgman}}$.'

A further, though subsidiary, advantage of this usage is in the explicit labelling of what could be called the 'intramural word.' We all know that, for pedagogical purposes, we may use words within our own classrooms in a manner that we never would in a formal publication. These may be purely heuristic words, the slang of the moment, exaggerations humorous or whimsical, or paradoxical definitions which could never endure serious examination. Such words can be very useful nevertheless, for they may help to set the stage for a serious discussion, or may be used to enlist interest and attention to what is to follow, or they may indicate that the discussion to follow, though narrow, is set against a broad background, etc. As an illustration, I recall a most interesting lecture in which the speaker's first sentence was "Psychiatry is the art of applying a science which does not yet exist."

From this usage, which all teachers (and all good conversationalists) know and use freely, may be derived the con-

cept of an 'intramural word,' that is, a word which has only arbitrary, momentary and local meaning, *i.e.*, only in this conversation, only at this time, in this place and for these people. To avoid producing confusion in the occasional student who is so humorless that he takes the usage quite seriously, and to avoid the embarrassment of having students quote such usages outside the classroom to people who cannot understand them, I have found it useful to append the subscript '$_{Int}$' both in lecturing and in writing. The same thing could be done by the psychologist using an 'intramural' word in the course of a single lecture or paper, for the sake only of temporary communication with the reader. He can thereby explicitly disavow any intention to propose the word seriously for inclusion in the general glossary of psychology.